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In This Issue

New research on the political and economic history of Europe is featured in this issue. We begin with a study of the French Revolution (on which a special issue in honor of the bicentennial is planned for the December *AHR* of this year) and move back through the crisis of the seventeenth century in Prussia to the Middle Ages in Western Europe and Russia.

The revisionist assault led by François Furet on Marxist interpretations of the French Revolution minimizes or denies the influence of social and ideological differences between the Nobles and the Third Estate during the first year of the revolution and stresses the early decline of the Nobles as a force in the National Assembly. **Timothy Tackett** reexamines these propositions and finds, on the contrary, that the representatives of privilege were a dynamic presence from the very beginning and that they introduced many of the electoral tactics usually attributed to the Jacobins. He also demonstrates that the social composition of the Right differed dramatically from that of the Left. Far from being of little consequence, the social and political conflict between Left and Right, according to Tackett, imparted a revolutionary dynamic to the Assembly from its inception to the middle of 1790.

Another attack on received opinion comes in our second article. **William W. Hagen** criticizes the “compromise thesis” of Hans Rosenberg, F. L. Carsten, and others, which holds that Prussian absolutism arose after the Thirty Years’ War in an agreement by which the Elector secured the political subordination of the nobles by granting them unrestricted authority over the peasants. Hagen argues that the country did not suffer a structural crisis before the war, the effects of the war allowed labor to command high wages, postwar legislation (previously thought to be important in establishing landlord control) was not as favorable to nobles as has been assumed, peasants had some leverage after the war due to continuing labor shortages, and this circumstance combined with increased state taxation to limit the rents going to landlords.

James Given looks beyond the story of the Cathar community told by Emmanuel Le Roy Ladurie in *Mountailou* to the methods of the inquisitors of the thirteenth and fourteenth centuries. He sees their effectiveness as a product not, as has been believed, of increasing bureaucratization but of the skillful application of technical resources available at the time. Among the tools employed by the inquisitors of Languedoc in their campaign to eliminate the Cathar heresy were collation of written testimonies, imprisonment of recalcitrant witnesses, and the creation of stigmatized social groups. Given understands power not as the expression of a “rational” institutional structure but as the art of using available means to manipulate social relations so as to disorganize enemies and mobilize the support of potential allies.

David B. Miller employs a novel method for assessing economic trends in medieval Russia: the quantitative analysis of monumental construction, principally the evidence of church, fortress, and town wall building in masonry and stone. His method, which includes comparison of developments in all major principalities of Russia, sheds new light on the relative importance of Northern Rus’ in the pre-Mongol period, the economic impact of the Mongol invasion and the Black Death plagues, and the movement of economic trends in the two centuries of Mongol rule in Northern Rus’.

In a review article, **Charles T. Wood** examines the renewal of interest in medieval politics. He points out that political history of the Middle Ages first took form in the context of nineteenth-century notions of national self-realization and therefore stressed the growth of the nation state and the bureaucratic institutions that seemed to sustain it. More recent work looks at the role of families and points out that what once looked like steps in the state-building process are better explained as the actions of kings operating as the heads of royal families. Personal relations long continued to be more important than institutional development. The creation of the nation state has ceased to be the key issue; the focus has turned to the growth of kingship with the administrative skill to wrest high returns from limited resources.

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Contributors

James Given is an associate professor of history at the University of California, Irvine, where he has taught since 1984. He studied medieval history with Gavin Langmuir at Stanford University, where he received the Ph.D. in 1976. He is the author of *Society and Homicide in Thirteenth-Century England* (1977). His current interests are the social determinants of political behavior in Western Europe between 1200 and 1500. He is working on two projects, one a comparative study of the effects of conquest and outside rule on North Wales and Languedoc, the other an examination of the role of politics and the state in late medieval Europe.

William W. Hagen, University of Chicago Ph.D., 1971, is professor of history at the University of California, Davis. He is the author of *Germans, Poles, and Jews: The Nationality Conflict in the Prussian East, 1772-1914* (1980). More recently, he has written a cycle of articles, including the essay in this issue, on landlord-peasant relations (and their significance in the process of state-formation) in early modern Brandenburg-Prussia. He is now writing a book on village life, noble lordship, and absolutist government in the Brandenburg countryside in the eighteenth and early nineteenth centuries.

David B. Miller is Seymour N. Logan Professor of History at Roosevelt University, Chicago,

where he teaches Russian and European history. Educated at the University of Wisconsin and Columbia University, he is author of *The Velikie minei cheti and the Steppennaia kniga of Metropolitan Makarii of Moscow and the Origins of Russian National Consciousness* (1979), and articles on medieval Rus' and early modern Russia. The present article grew out of a larger study in progress on the transformation of Rus' in the thirteenth and fourteenth centuries.

Timothy Tackett is a professor of history at the University of California, Irvine. He received his doctoral degree at Stanford University in 1973, with his dissertation directed by Philip Dawson, and has taught previously at Marquette University, the Catholic University of America, and the Université de Rennes II (France). He has published a number of articles and two books: *Priest and Parish in Eighteenth-Century France* (1977) and *Religion, Revolution, and Regional Culture* (1986). At present, he is writing a general social and political study of the Constituent Assembly and, with Claude Langlois, a book about revolutionary religion in France, 1789-1799.

Charles T. Wood is Daniel Webster Professor at Dartmouth College and a Fellow of the Medieval Academy of America. His most recent book is *Joan of Arc and Richard III: Sex, Saints, and Government in the Middle Ages* (1988).

Nobles and Third Estate in the Revolutionary Dynamic of the National Assembly, 1789–1790

TIMOTHY TACKETT

FOR OVER TWO DECADES NOW, DEBATE HAS RAGED between “Marxists” and “revisionists” over the question of the French Revolution.¹ The outlines of this debate have become familiar even to historians with no particular expertise in eighteenth-century French studies. In place of the Marxist or Marxist-inspired vision of a revolution arising out of class conflict between nobility and bourgeoisie, most revisionists would stipulate a revolution “caused” ultimately by the internal collapse of the monarchy. In this view, the nobles and the upper-class commoners were converging in the late eighteenth century into a single “elite” group, bound by common economic interests and cultural experiences and by the substantial possibilities of social mobility into the nobility. When the two groups fell into conflict in 1789, it was either a kind of accidental aberration arising from misunderstandings, a difference in “style,” or a failure of imagination and leadership.

Although most of the controversy to date has hinged on the question of revolutionary origins, François Furet, the leading French representative of revisionism, has also pushed a reconsideration of the revolutionary dynamic after the opening of the Estates General in May 1789. In his widely read and influential book, *Penser la Révolution*, Furet argued that, once the revolution had begun, it was impelled forward through the workings not of a class struggle but of a power struggle.² By June of 1789 and the creation of the National Assembly,

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¹ Perhaps the best general summaries of the debate are in William Doyle, *Origins of the French Revolution* (Oxford, 1980), 7–40; Lynn Hunt, *Politics, Culture, and Class in the French Revolution* (Berkeley, Calif., 1984), 3–10; and D. M. G. Sutherland, *France, 1789–1815: Revolution and Counterrevolution* (Oxford, 1985), 15–18. See also the recent articles by Michael P. Fitzsimmons, “Privilege and the Polity in France, 1786–1791,” *AHR*, 92 (April 1987): 269–95; and Michel Vovelle, “L’historiographie de la Révolution française à la veille du bicentenaire,” *Annales historiques de la Révolution française*, 272 (1988): 113–26. Vovelle would substitute “Jacobins” for “Marxists,” since not all historians embracing the synthesis of Jean Jaurès and Georges Lefebvre were themselves Marxists. There is also, of course, a wide variety of nuances and approaches among the various scholars in the “revisionist” line.

² François Furet, *Penser la Révolution* (Paris, 1978). In citations, I will use the English version, *Interpreting the Revolution*, trans. Elborg Forster (Cambridge, 1981). Translated into several lan-

the privileged orders, like the monarchy itself, had essentially “capitulated,” and, by October, as Furet and Denis Richet wrote elsewhere, “the battlefield had essentially been conquered, the fight was over: the revolution had been won.”³ Thereafter, conflict within the National Assembly pitted various elements of the Third Estate against one other. The revolution was progressively democratized and radicalized as successive factions of Patriots each claimed to be the authentic voice of popular sovereignty, the true mouthpiece of the general will. Political struggle thus became a battle of rhetoric and of ideology—but with no class content. It also became a battle of denunciations, as each faction tried to outdo its opponents in its condemnations of “aristocratic plots” and counterrevolutionary conspiracies. But, in Furet’s view, these denunciations were largely contrived and the plots “imaginary,” “the figment of a frenzied pre-occupation with power,” and the indication also of an incipient terrorist mentality in evidence among the Patriots as early as 1789.⁴

But did the “aristocrats” really capitulate and abandon the political struggle so soon? And were early revolutionary developments so totally devoid of social dimensions? I have no intention of considering Furet’s complex and suggestive thesis in all its ramifications, or of attempting to treat every aspect of the revolution. Here, I would only present some of the results of recent research into factional organization and the revolutionary dynamic at the vital core of political life during the early revolution, the Constituent Assembly. In fact, despite the revisionist call for a return to politics—and despite the awesome number of studies devoted to this period in French history—the internal political life of France’s first National Assembly is still rather poorly understood. This is true in part for historiographical reasons. Interest in the process and functioning of the Constituent Assembly has frequently been overshadowed by historians’ tenacious fascination with the problem of the origins of the Republic and the Terror. But research has also been hampered by difficulties with sources. The official accounts of events within the assembly halls are often incomplete and tendentious. No minutes at all were maintained by the Third Estate through the second week in June,⁵ and, even after the appearance of official record keeping, minutes were commonly sanitized and abridged by the secretaries in power to

guages, Furet’s work has become standard reading in university courses on the revolution and provided a central theme for discussion in a recent international conference. See also Keith Michael Baker, ed., *The French Revolution and the Creation of Modern Political Culture*, vol. 1, *The Political Culture of the Old Regime* (Oxford, 1987), esp. the introduction by Baker; and Jack R. Censer, “The Coming of a New Interpretation of the French Revolution,” *Journal of Social History*, 21 (1987): 295–309. Among the few other revisionist or quasi-revisionist works treating the revolutionary period itself, see especially Patrice Higonnet, *Class, Ideology, and the Rights of the Nobles during the French Revolution* (Oxford, 1981); and Norman Hampson, *Prelude to Terror: The Constituent Assembly and the Failure of Consensus* (Oxford, 1988).

³ Furet, *Interpreting the Revolution*, 46; François Furet and Denis Richet, *La Révolution française* (Paris, 1973), 99.

⁴ Furet, *Interpreting the Revolution*, 54.

⁵ The first official minutes of the Third Estate seem to have been taken on June 12; *Archives parlementaires, Première série*, Jérôme Mavidal, et al., eds., 82 vols. (Paris, 1867–1913), 8:94. (Hereafter, *AP.*) The early minutes were partially reconstructed several weeks later.

promote a desired public impression of assembly activities.⁶ Moreover, the near absence of nominal roll-call votes—the principal meat of what once was called the “new” history of parliamentary behavior—renders the careful quantitative assessment of deputy alignments all but impossible.⁷ Nevertheless, if it is not feasible to reconstitute voting records or follow the manifold, day-to-day fluctuations of every deputy, one can at least take note of such glimpses of collective behavior as revealed by the lists of adhesions to political clubs and the signatures on petitions.⁸ One can also make use of the incomplete records of the periodic elections of National Assembly officers—presidents, secretaries, and committee members. And, perhaps most important, one can examine the considerable number of accounts—many of them still only in manuscript—written by the deputies themselves in letters and memoirs to their families and home constituencies.⁹

THE EARLIEST FORMATION OF POLITICAL GROUPINGS within the Estates General will probably always remain somewhat uncertain.¹⁰ Yet the most important and influential of these could clearly trace their genealogies to the prerevolutionary period. Within days of their arrival in Versailles, groups of liberal deputies of the second estate were regularly congregating at the residences of the Duc de La Rochefoucauld or the Marquis de Montesquiou or in the “Viroflay Society” on the estates of the Duc de Piennes.¹¹ A substantial number of the participants, perhaps the majority, were veterans of the Paris-based association of Patriots, the so-called Society of Thirty—from whose membership no fewer than twenty-six

⁶ See, for example, the Assembly’s decision on April 29, 1790, to expunge the Comte de Virieu’s speech resigning the presidency because it was deemed “injurieuse” to the Assembly; *Réimpression de l’ancien Moniteur (mai 1789–novembre 1799)*, 31 vols. (Paris, 1858–63), 4:241.

⁷ Motions to create such lists were rejected on July 9, August 29, and again on December 8, 1789; AP, 8:510 and 10:776–77; Philip Dawson, *Provincial Magistrates and Revolutionary Politics in France, 1789–1795* (Cambridge, Mass., 1972), 194; and Jean-François Gaultier de Biauzat in Francisque Mège, ed., *Gaultier de Biauzat, député du Tiers-Etat aux Etats-Généraux de 1789: Sa vie et sa correspondance*, 2 vols. (Clermont-Ferrand, 1890), 2:168.

⁸ The present study is based on a new computerized prosopography of the 1,315 deputies who sat at any time during the Constituent Assembly. See also the works of Harriet B. Applewhite: “Political Legitimacy in Revolutionary France, 1788–1791,” *Journal of Interdisciplinary History*, 9 (1978): 245–73; and “Political Alignment in the French National Assembly, 1789 to 1791,” *Proceedings of the Annual Meeting of the Western Society for French History*, 8 (1980): 265–75. For a complementary study based on a linguistic analysis of newspaper accounts, see Pierre Rétat, “Partis et factions en 1789: Emergence des désignants politiques,” *Mots*, 16 (1988): 69–89.

⁹ To date, I have located memoirs, letters, or diaries describing the events of the Constituent Assembly, written by a total of 114 deputies. About half of these are in manuscript. Edna Hindie Lemay has consulted some of these materials in her *La vie quotidienne des députés aux Etats généraux, 1789* (Paris, 1987).

¹⁰ Among the few general studies, see Rudolf Von Albertini, “Parteiorganisation und Parteibegriff in Frankreich, 1789–1940,” *Historische Zeitschrift*, 193 (1961): esp. 529–46.

¹¹ See Alexandre Lameth, *Histoire de l’Assemblée constituante*, 2 vols. (Paris, 1828–29), 1:34–36; G. Michon, *Essai sur l’histoire du parti Feuillant: Adrien Duport* (Paris, 1924), 48; Augustin Challamel, *Les clubs contre-révolutionnaires, cercles, comités, sociétés, salons, réunions, cafés, restaurants, et librairies* (Paris, 1895), 129, 133; Charles Du Bus, *Stanislas de Clermont-Tonnerre et l’échec de la Révolution monarchique, 1757–92* (Paris, 1931), 84.

had successfully sought election to the Estates General.¹² Most of these men were Parisians who had long known each other and who were linked through a dense network of association in Masonic lodges, mesmerist groups, and a variety of Enlightenment and philanthropic societies. Many nobles of the sword in the group were also bound together as outsiders to the clique of courtiers then in favor in Versailles.¹³ With considerable previous experience in political organization, young noblemen such as Adrien Duport, Charles and Alexandre de Lameth, the Marquis de Lafayette, and the Comte de Clermont-Tonnerre mapped the strategy that ultimately led to the secession of the liberal nobles from their order and their union with the Third Estate.¹⁴

Several members of the “Commoners”—Antoine Barnave, Jean-Paul Rabaut-Saint-Etienne, Abbé Emmanuel-Joseph Sieyès, for example—seem also to have attended meetings of the liberal noblemen.¹⁵ But the most celebrated and influential of the early Versailles clubs was in fact indigenous to the Third Estate. There is no need to repeat here the well-known history of the Breton Club.¹⁶ It should be emphasized, however, that many and perhaps most of the Breton deputies had previously participated in provincial estates or in the local committees that had coordinated opposition against the privileged orders and their attempts to dominate Breton affairs.¹⁷ Even before arriving in Versailles, the newly elected Third Estate delegations had met in Rennes to discuss strategy, and many of the deputies then traveled to the capital together. After the opening of the Estates General, they followed a procedure apparently already practiced during the last Estates of Brittany, debating important issues in a café each evening, arriving at a majority decision, deciding who would speak the next day in favor of that decision, and urging all participants to vote as a bloc. It was also the specific provincial context—coupled with the absence of all nobles and bishops from the delegation—that rendered the Breton representatives so

¹² Daniel L. Wick, *A Conspiracy of Well-Intentioned Men: The Society of Thirty and the French Revolution* (New York, 1987), 342–47, 354–55. The list given by Wick on 354–55 includes one *suppléant* deputy and overlooks the Duc de Luxembourg. The “thirty” seem actually to have consisted of about fifty-five individuals.

¹³ See Ran Yadid-Halévi, “La sociabilité maçonnique et les origines de la pratique démocratique” (thèse de 3^e cycle, Paris, Ecole des Hautes Etudes en Sciences Sociales, 1981). Among the known Mesmer enthusiasts were Nicolas Bergasse, Jean-Jacques Duval d’Eprémesnil, Pierre-Victor Malouet, François-Dominique de Reynaud de Montlosier, Adrien Duport, Jérôme Pétion de Villeneuve, Jean-André Périsset-Duluc, the Marquis de Lafayette, the Duc de Coigny, and perhaps the Comte de Virieu. See Robert Darnton, *Mesmerism and the End of the Enlightenment in France* (New York, 1970), 68, 74, 78–79; Etienne Lamy, *Un défenseur des principes traditionnels sous la Révolution: N. Bergasse* (Paris, 1910), 46; François-Dominique de Reynaud de Montlosier, *Mémoires*, 2 vols. (Paris, 1830), 1:133, 324–27; Pierre-Victor Malouet, *Mémoires*, 2 vols. (Paris, 1874), 1:195. The Viroflay Society was said to have had a distinctly Masonic character. See also Wick, *Conspiracy of Well-Intentioned Men*, 49, 90–99; and Daniel L. Wick, “The Court Nobility and the French Revolution: The Example of the Society of Thirty,” *Eighteenth-Century Studies*, 13 (1979–80): 263–84.

¹⁴ Lameth, *Histoire de l’Assemblée constituante*, 1:420–21; Michon, *Essai sur l’histoire du parti Feuillant*, 48–52.

¹⁵ Michon, *Essai sur l’histoire du parti Feuillant*, 48.

¹⁶ See, for example, F.-A. Aulard, *La Société des Jacobins*, 6 vols. (Paris, 1889–97), 1: “Introduction”; Gérard Walter, *Histoire des Jacobins* (Paris, 1946), 1–55; A. Bouchard, *Le club breton: Origine, composition, rôle à l’Assemblée constituante* (Paris, 1920); and Lemay, *La vie quotidienne*, part 2, chap. 4.

¹⁷ Bouchard, *Le club breton*, 21–23.

exceptionally radical.¹⁸ From the earliest days of the Estates General, they advocated a unilateral transformation of the Third Estate into a “salle nationale” or “assemblée nationale”—anticipating by some six weeks the famous motions by Abbé Sieyès to this effect.¹⁹ Indeed, for many in the Breton group, such a strategy was conceived not as a ploy for forcing joint deliberation with the Nobles and Clergy but as the first step in creating a constitution *without* the participation of the privileged orders.²⁰ They soon came to be identified with an unrestrained hatred of all nobles: “an extreme violence,” “an implacable hatred of the nobility,” as two of the more moderate deputies described it.²¹

Yet the Bretons’ rapid rise to prominence in early June was by no means inevitable and was rarely anticipated by contemporaries. Although the Breton delegation began inviting representatives from other provinces to participate in its meetings early on, many deputies expressed their aversion and mistrust of the very idea of factions or “cabals” or an “esprit de parti,” widely viewed as warping the representative process. The Lorraine landholder and sometime scholar Adrien Duquesnoy spoke harshly of the Breton delegation as “hotheads without measure and without moderation.” For the Bordeaux merchant Pierre-Paul Nairac, they were “always moving toward extreme positions,” while the Alsatian Etienne-François Schwendt sharply criticized them for attempting to “exercise a kind of domination over all opinions.”²² Others were clearly disconcerted by the Bretons’ abrasive attitude toward the nobility. In this, they reflected the fears and ambiguities of men who were often socially and juridically at the very frontier between noblemen and commoners. The majority had probably spent many years of their lives imitating aristocratic values and patiently working within the aristocratic system. Whatever their views—and perhaps rage—against the injustice or irrationality of such a society, few had even dreamed that the system and its values could themselves be changed.²³ In any case, on May 18, the

¹⁸ Walter, *Histoire des Jacobins*, 12–15; Bouchard, *Le club breton*, 21–22; Lemay, *La vie quotidienne*, 212–13; Jean-Pierre Boullé, “Ouverture des Etats généraux de 1789,” ed. Albert Macé, *Revue de la Révolution: Documents inédits*, 10 (1887): 162–65; Léon Dubreuil, “Le clergé de Bretagne aux Etats généraux,” *La Révolution française*, 70 (1917): 483–84. Joseph Delaville Le Roux to Municipality of Lorient, May 15, 1789, BB 12, Archives Communales de Lorient (hereafter, AC, Lorient), describes the voting procedures followed. The Breton nobles and upper clergy had boycotted the elections in the spring of 1789 and had consequently sent no deputies to Versailles.

¹⁹ Delaville Le Roux, May 3 and 8, 1789, BB 12, AC, Lorient; Boullé, *Revue de la Révolution*, 10 (1887), 169. Sieyès had proposed an “Assemblée nationale” the previous January. See his *Qu’est-ce que le Tiers Etat?*, ed. Edme Champion (Paris, 1889), 79.

²⁰ Reported by Antoine Durand to Delcamp-Boytré in Gourdan, May 6, 1789, carton 5–56, Archives diocésaines de Cahors; François-René-Pierre Ménard de la Groye to his wife, May 19, 1789, 10 J 122, Archives Départementales de la Sarthe (hereafter, AD, Sarthe); and Jacques-Antoine Creuzé-Latouche, *Journal des Etats généraux et du début de l’Assemblée nationale, 18 mai–29 juillet 1789*, ed. Jean Marchand (Paris, 1946), 130.

²¹ Adrien Duquesnoy, *Journal d’Adrien Duquesnoy*, ed. R. de Crèvecoeur, 2 vols. (Paris, 1894), 1:13; Laurent de Visme, May 14, 1789, ms. “Journal des Etats généraux,” Nouv. acq. fr. 12938, Bibliothèque Nationale.

²² Duquesnoy, *Journal*, 1:2; Pierre-Paul Nairac, May 19, 1789, ms. “Journal,” 5 F 63, Archives Départementales de l’Eure; Etienne-François Schwendt in Rodolphe Reuss, ed., *L’Alsace pendant la Révolution française*, 2 vols. (Paris, 1880–94), 1:108. See also Gaultier de Biauzat, *Correspondance*, 2:118; and Jean-Baptiste Poncet-Delpech, *La première année de la Révolution vue par un témoin*, ed. Daniel Ligou (Paris, 1961), 11–12.

²³ Many remained wary of the nobles’ power and fearful of jeopardizing even minimal gains by overly bold action. Thus the Poitevin deputy René-Antoine-Hyacinthe Thibaudeau had warned his

motion by the Breton Isaac-René-Guy Le Chapelier, declaring that the deputies of the "National Assembly" did not represent specific orders but the whole nation, seems to have won the support of only sixty-six deputies—of whom forty-three were from Le Chapelier's own provincial delegation.²⁴

The spectacular success of the Bretons in early June was probably the result of a number of factors. But, to believe the letters and diaries of the deputies themselves, no single factor was more important than the growing intransigence of the majority of the privileged deputies and their ultimate refusal to consider compromise or reconciliation in any form over the crucial question of voting procedure. For, in point of fact, the defenders of privilege and tradition among the deputies, no less than the deputy-Patriots, had also begun organizing in support of their positions. By the end of May—and probably a good deal earlier—bishops in the first estate were meeting with some regularity in the church of Notre-Dame in Versailles. Many of the prelates had known one another for years and were linked by ties of family as well as by a common educational experience at the seminary of Saint-Sulpice.²⁵ They readily reactivated a miniature version of the General Assembly of the Clergy—on which most of them had long collaborated—and they were notably effective in countering the activities of the more liberal curé-deputies, frustrating their efforts to unite with the Third Estate until after the unilateral decision of that estate to verify credentials in common.²⁶

Unlike the Clergy, the conservative and reactionary Nobles—the great majority within the order—had no real institutional base on which to build an effective organization. Nevertheless, a substantial number of the Nobles undoubtedly knew one another prior to the convocation of the Estates General. Historians have not previously noted that almost 40 percent of the Nobles were actually residents of Paris who had scattered into the provinces to seek election in districts where their families owned land and seigneuries.²⁷ At least 78 percent

son earlier against "un acte de hardiesse qui n'aurait eu d'autre effet que de nous attirer à dos le clergé et la noblesse." And Durand would recall to a friend how cautious and prudent they had to be in drawing up their cahiers: "Il fallait encore se garder de trop alarmer le despotisme"; Durand, September 4, 1789, carton 5–56, Archives diocésaines de Cahors; Antoine-Claude Thibaudeau, *Biographie, Mémoires, 1765–92* (Paris, 1875), 64. Research on the social origins of the Third Estate deputies will be published subsequently. See also Edna Hindie Lemay, "La composition de l'Assemblée nationale constituante: Les hommes de la continuité," *Revue d'histoire moderne et contemporaine*, 24 (1977): 340–63; and Colin Lucas, "Nobles, Bourgeois, and the Origins of the French Revolution," *Past and Present*, 60 (August 1973): 90.

²⁴ Michel-René Maupetit, "Lettres de Michel-René Maupetit," ed. E. Quériau-Lamerie, in *Bulletin de la Commission historique et archéologique de la Mayenne*, 2^e série, 18 (1902): 136. Le Chapelier's motion was made on May 13; *AP*, 8:36–37. See also the Vicomte de Mirabeau's interpretation of this motion; *AP*, 8:42.

²⁵ Michel Peronnet, *Les évêques de l'ancienne France* (Lille, 1977), esp. 1337–43.

²⁶ The first curés came over to the Third Estate on June 13. Many had originally hoped to join much earlier. See my article, "Les députés du premier ordre: Le Clergé aux Etats généraux et à l'Assemblée constituante, 1789–1791," in *Croyances, pouvoirs et sociétés: Des Limousins aux Français: Etudes offertes à Louis Pérouas* (Treignac, 1988), 85–99.

²⁷ Based on *Almanach de Paris, Première partie, contenant les noms et qualités des personnes de condition pour l'année 1789* (Paris, 1789). Only about 20 percent of the Nobles were country gentlemen known to have resided in their chateaus. See also James Murphy and Patrice Higonnet, "Les députés de la noblesse aux Etats généraux de 1789," *Revue d'histoire moderne et contemporaine*, 20 (1973): 230–47.

of the Nobles had been educated for the military and were or had previously been commissioned officers in the army or navy. Close to two-thirds, moreover, could trace their families back to at least the sixteenth century, while over half could apparently prove lineage dating to the fourteenth century or earlier.²⁸ They represented most of the great families of France, and many were closely related to one another—and to the equally aristocratic episcopal families—and had long associated with one another at court and in Parisian societies. In short, the Nobles of 1789 were an extraordinarily “aristocratic” body in the full sense of the word—considerably more so than their counterparts in the Estates General of 1614. As a corps, they occupied a dramatically different sphere of status and prestige—and probably of wealth—from that occupied by the Third Estate.²⁹ Although we know relatively few details about the political workings of the Nobles during this period, the substantial blocs of votes given to the winning candidates in the secret ballots for officers of the order in June 1789 strongly suggest some measure of organization.³⁰ Indeed, the Marquis de Ferrières spoke of the “club” of conservative noblemen, led by Jean-Jacques Duval d’Eprémèsnil—judge in Parlement and renegade ex-associate of the Committee of Thirty—the Marquis de Bouthillier, and the Vicomte de La Queue.³¹ With the strong support and patronage of the Comte d’Artois and the reactionary court faction, this group proved remarkably successful through the end of June in maintaining the disciplined intransigence of the great majority of noble deputies. Indeed, several of the originally “liberal-leaning” noblemen, including some with statements of grievances (*cahiers de doléances*) mandating a vote by head in the Estates General, are known to have been won over to the hard-line position.³²

In any case, many of the Third Estate deputies became increasingly convinced

²⁸ For military experience, I have used Armand Brette, *Recueil de documents relatifs à la convocation des Etats généraux de 1789*, 4 vols. (1894–1915), vol. 2; and Adolphe Robert, et al., *Dictionnaire des parlementaires français*, 5 vols. (Paris, 1891). For the approximate dates of ennoblement, I have used Henri Jouglé de Morenas, *Grand armorial de France*, 6 vols. (Paris, 1934–49); and François Bluche, *Les honneurs de la cours* (Paris, 1957). More precise conclusions must await the biographical dictionary of the Constituent Assembly to be edited by Edna Lemay. Such proportions would, of course, have been largely similar for the liberal nobility. Within this group, as we shall see below, age was probably the critical factor.

²⁹ J. Michael Hayden, *France and the Estates General of 1614* (Cambridge, 1974), esp. 82–85. See also Sutherland, *France, 1789–1815*, 19–21.

³⁰ The Duc de Luxembourg won on the first ballot for president, with 145 out of 251. The Duc du Croy won the vice-presidency with 160 out of 239; the Marquis de Bouthillier was elected secretary with 139 out of 184. See Ambroise-Eulalie de Maurès de Malartic, June 12, 15, and 16, 1789, “Journal de ma députation aux Etats généraux,” MS. 21, Bibliothèque Municipale de La Rochelle (hereafter, BM, La Rochelle).

³¹ Marquis de Ferrières, *Mémoires*, 3 vols. (Paris, 1825), 1:37, 60.

³² Among those won over to a position of intransigence were the Marquis de Guilhem-Clermont-Lodève and the Duc de Châtelet: Guilhem to the Municipality of Arles, May 21, 1789, AA 23, Archives Communales d’Arles. Note also the interpretation of the Marquis de Ferrières, *Mémoires*, *passim*. Malartic claimed that, on June 25, six noble deputies with mandates for votes by head were refusing to join the Third Estate and were writing home for new mandates supporting their intransigence: see Malartic’s entry for that date, “Journal,” MS. 21, BM, La Rochelle. Of the twenty-seven Noble deputies from *bailliages* indicated by Guy Chaussinand-Nogaret to have had imperative mandates for a vote by head, only fifteen actually joined the Third Estate before June 27; *La noblesse au XVIII^e siècle* (Paris, 1976), 184–85; and Murphy and Higonnet, “Les députés de la noblesse,” 244–46.

of the threat of such organization to the reforming desires of the nation. The Breton deputy Jean-Pierre Boullé warned his constituents on June 9 that the “aristocratic committee” led by Eprémèsnil was meeting daily to plot its strategy and that organized action by the Patriots was necessary if the “desires of the nation” were ever to triumph.³³ Even many of the normally moderate and prudent deputies—“les hommes sages,” as they liked to call themselves—began commenting on the hopeless resistance to compromise on the part of the bishops and Nobles. With the “aristocrats” of the first and second estates rejecting any form of conciliation, the Third ultimately had no choice, it was argued, but to pull together and go it alone. The self-consciously moderate Antoine Durand, a lawyer from Cahors, was outraged by the Nobles’ statement of May 28, which accepted royal mediation but rejected in advance any discussion of a vote by head: the Nobles “refuse to yield an inch of ground.” “Those who have led the Nobles,” wrote the usually cautious judge Jean-Baptiste Grellet de Beauregard, “have blocked all roads to compromise”; while his colleague from Toul, the *lieutenant général* Claude-Pierre Maillot, concluded that “the violence of the Nobles’ decisions have increased rather than weakened the determined resolution of the Third.” The wealthy landholder and mayor of Laon, Laurent de Visme, noted in his diary that under normal circumstances he would never have accepted Sieyès’s motion of June 10, but now he was inclined to do so: “The Nobles’ actions have justified it.”³⁴

The Breton Club probably reached the pinnacle of its ascendancy toward the middle and end of June. Boullé wrote on June 9 that “during the last few days our salon has become the rallying point for all good citizens . . . All the best citizens from all of the provinces are assembling there.” On that evening, the Breton group seems to have sent out delegates to argue its case within the individual *bureaux*—the small discussion groups into which the Assembly had recently divided itself—thus measurably contributing to the development of opinion in favor of the motion to be voted on the next day.³⁵ To be sure, one should not underestimate the role played by the Abbé Sieyès himself, whose prestige and eloquent articulation of revolutionary objectives had an enormous impact on many of the deputies.³⁶ But, in fact, each of Sieyès’s motions seems to have been discussed and debated in the Breton Club before being brought to the full assembly of the Third Estate, and the principles in question had been

³³ Boullé, *Revue de la Révolution*, 12 (1888), 50.

³⁴ Durand, May 30, 1789, carton 5–56, Archives diocésaines de Cahors; Jean-Baptiste Grellet de Beauregard, “Lettres de M. Grellet de Beauregard,” ed. Abbé Dardy, *Mémoires de la Société des sciences naturelles et archéologiques de la Creuse*, 2^e sér., 7 (1899): July 10, 1789; Claude-Pierre Maillot to an unnamed municipal official of de Toul, June 3, 1789, JJ 7, Archives Communales de Toul (hereafter, AC, Toul); and Visme, “Journal des Etats généraux,” May 26 and June 10, 1789. All four men were moderates who never joined the Jacobin Club. See also Pierre-Joseph Meiffrund, June 10, 1789, ms. journal; copy in Institut de la Révolution française (Paris).

³⁵ Nairac, “Journal,” June 9, 1789, 5 F 63, Archives Départementales de l’Eure; Boullé, *Revue de la Révolution*, 12 (1888), 40, 49. Visme also described “une grande conférence tenue par les Bretons et leurs partisans”; “Journal des Etats généraux,” June 10, 1789.

³⁶ See Maupetit, “Lettres,” 18 (1902): 157–58.

continuously advocated by the Breton delegation for well over a month.³⁷ Once viewed with considerable mistrust by the majority of deputies, the Breton “committee” now became the center of all political activity in the Third Estate. Impelled by the absolute intransigence of the Nobles and the apparent deadlock of the Clergy, buoyed and invigorated by the support of the Versailles crowds, the Third Estate achieved a remarkable consensus around the Tennis Court Oath and the revolutionary declarations of June 10, 17, and 23: a new definition of sovereignty and political legitimacy in open defiance of the monarchy that would probably have seemed impossible or unthinkable to most of the deputies just a few weeks earlier.³⁸

BY ALL ACCOUNTS, THE PERIOD FROM LATE JUNE TO EARLY AUGUST witnessed a substantial transformation of the political chemistry of the Assembly, a restructuring of many of the positions and alliances of the middle of June. Two developments in particular seem to have contributed to cracking the solidarity and apparent consensus of the nascent National Assembly. The first was the popular violence that exploded in the capital in mid-July but that continued in both Paris and the provinces well into August. In short order, the image of the “people” held by many of the deputies—as revealed in their letters and diaries—was dramatically altered. The Rousseauist conception of the Common Man as repository of goodness and truth was frequently replaced, or at least strongly modified, by the image of the violent, unpredictable, and dangerous classes of July and August.³⁹ To judge by the deputies’ own writings, the most shocking event was usually not the storming of the Bastille on July 14 but the popular executions about a week later of the royal officials Joseph Foulon de Doué and Louis Berthier de Sauvigny, the details of which were luridly recounted in the Assembly by deputies who had witnessed them.⁴⁰ Although several of the

³⁷ Walter, *Histoire des Jacobins*, 22; Henri Grégoire, *Mémoires*, ed. H. Carnot, 2 vols. (Paris, 1837–40), 1:380. According to Gaultier de Biauzat, the motion had already been decided by the Breton group when it was voted to have Sieyès present it: “pour donner plus de poids à la motion”; Gaultier de Biauzat, *Correspondance*, 102.

³⁸ There was a near-perfect consensus in the Tennis Court Oath of June 20. While eighty-nine Third Estate deputies voted against Sieyès’s motion on June 17, this was primarily a disagreement over the specific name to be given to the new assembly. On the very next vote that day, the National Assembly unanimously decided that all taxes would be subject to reorganization; see, for example, Jean-Sylvain Bailly, *Mémoires*, 3 vols. (Paris, 1821–22), 1:179–80; Maupetit, “Lettres,” 19 (1903): 215; Nairac, “Journal,” June 17, 1789, 5 F 63, Archives Départementales de l’Eure; and Meiffrund, ms. journal, June 17, 1789, Institut de la Révolution française. The evidence does not seem to sustain Georges Lefebvre’s assertion that the eighty-nine deputies “disavowed the juridical Revolution”; *The Coming of the French Revolution* (Princeton, N.J., 1947), 82.

³⁹ In general, the Constituent deputies’ attitudes toward the people were very complex. But, through the end of June, the Third Estate deputies’ views of the crowds in Versailles were almost universally favorable; see, for example, Jean-Baptiste Poncet-Delpech, “Documents sur les premiers mois de la Révolution,” ed. Daniel Ligou, *Annales historiques de la Révolution française*, 38 (1966): 430; Duquesnoy, *Journal*, 1:133–34; Creuzé-Latouche, *Journal*, 25–26; Boullé, *Revue de la Révolution*, 11 (1888), 18. Afterward, many of the deputies, not only of the Clergy and the Nobles but of the Commoners as well, seem to have felt directly threatened. See, for example, Félix Faulcon, *Correspondance*, vol. 2, 1789–91, ed. G. Debien (Poitiers, 1953), journal entry of July 13, 1789; Grellet, “Lettres,” 76–77.

⁴⁰ AP, 8:263–67.

radicals revealed obvious sympathy for the past suffering of the people, and others were pushed by events toward a new, more expansive definition of the electorate they represented,⁴¹ the overall reaction was one of outrage and horror: “barbarous and atrocious violence” (Visme), “scenes of cruelty and horror” (the Third Estate deputies from Marseille), “arbitrary executions that arouse horror” (Joseph Delaville Le Roulx from Brittany).⁴² In any case, the July violence is known to have been a key factor in the movement of many deputies away from the more democratic vision of the new regime that had garnered increasing favor in June. It was certainly a major element in the changing position of the “Monarchien” Jean-Joseph Mounier and his liberal noble ally, the Comte de Clermont-Tonnerre. Looking back on this event, Clermont-Tonnerre wrote in 1791: “I feared that we were inciting atrocities; I remembered the St. Bartholomew’s Day Massacre . . . and I sadly asked myself, ‘Are we even worthy of being free?’”⁴³

The second development that contributed to breaking down the earlier consensus within the Third Estate was the entry of the privileged orders into the National Assembly. In point of fact, the process of integrating the first two estates into the Assembly was long and difficult. Even though a majority of the Clergy and a small minority of the Nobles came over to the Third of their own accord between June 22 and 26, the forced union of the remainder was nothing short of traumatic. Many of the recalcitrants were even ready to refuse the king’s request for union on June 27, and it was only after receiving a warning from the Comte d’Artois that the king’s life was in danger that they sullenly marched into the National Assembly, “tears in their eyes, and rage and despair in their hearts.”⁴⁴ For the next three weeks, a significant minority of both the Nobles and the Clergy continued to boycott all votes and discussions and returned daily to their own meeting halls, frequently voting formal protests of decrees made in the Assembly.⁴⁵ It was only after the Parisian insurrections of mid-July that the “parti protestant” agreed to take part in the proceedings.⁴⁶ “Severed heads,” as one caustic deputy remarked, “were frightfully instructive.”⁴⁷ But, even at that, it was early August before Jacques-Antoine-Marie de Cazalès and the Abbé

⁴¹ For examples, see Creuzé-Latouche, *Journal des Etats généraux*, 270–71; Ménard de la Groye, July 24, 1789, 10 J 122, AD, Sarthe; Maximilien Robespierre, *Correspondance*, ed. Georges Michon, 2 vols. (Paris, 1926–41), 50; François-Joseph Bouchette, *Lettres*, ed. C. Looten (Lille, 1909), 234.

⁴² Visme, “Journal des Etats généraux,” July 22, 1789; letter of the deputies of Marseille, July 27, 1789, BB 361, Archives Communales de Marseille; Delaville Le Roulx, July 24, 1789, BB 12, AC, Lorient. Numerous other examples could be given.

⁴³ Du Bus, *Stanislas de Clermont-Tonnerre*, 123. See also Jean Egret, *La révolution des notables: Mounier et les monarchiens* (Paris, 1950), 92–103; and Montlosier, *Mémoires*, 1:251.

⁴⁴ August-Félix-Elizabeth Barin de La Gallissonnière, June 27, 1789, ms. journal, A4 LVI, Archives de la Guerre; also Louis-Henri-Charles de Gauville, *Journal*, ed. Edouard de Barthélémy (Paris, 1864), 8. On the Nobles’ initial refusal to obey the king, see Malartic, “Journal,” June 27, 1789, MS. 21, BM, La Rochelle; and Jean-Baptiste de Cernon de Pinteville to his brother, undated letter of ca. June 27, J 2286, Archives Départementales de la Marne.

⁴⁵ La Gallissonnière, ms. journal, A4 LVI, folio 155, Archives de la Guerre; Durand, June 30, 1789, carton 5–56, Archives diocésaines de Cahors; Malartic, “Journal,” July 9 and 11, 1789, MS. 21, BM, La Rochelle; Delaville Le Roulx, July 11, 1789, BB 12, AC, Lorient.

⁴⁶ The term was used by Maupetit on July 11; “Lettres,” 18 (1902): 461.

⁴⁷ Maillot, July 18, 1789, JJ 7, AC, Toul. For a nobleman’s interpretation, see Malartic, “Journal,” July 16, 1789, MS. 21, BM, La Rochelle.

Jean-Siffrein Maury and several other of the most conservative deputies who had fled—and, in many cases, been chased back by their own constituencies—formally announced their intention of participating in the National Assembly.⁴⁸ Yet prolonged opposition of this kind was almost certainly not the norm. After the initial shock of June 27, the majority of the newly arrived clergy and noblemen seemed to adapt themselves to the situation with surprising grace. After a two-day break in late June, according to the Comte de La Gallissonnière, “the deputies had calmed down a bit and were less frightened, and a new order of things seemed to appear.”⁴⁹ The Baron de Pinteville described the sharp reversal in sentiment of many of his colleagues who had long held back because of fear and pride and pressure from the reactionary “party” but who were now swept by sentiments of patriotism and duty to the Nation: “all was forgotten as they came forward with this act of self-sacrifice.”⁵⁰ For the Marquis de Ferrières, the experience was a revelation: nervous at first, he soon discovered that he was far more at ease with the commoners of the Third Estate than with the great court nobles, whom he had always detested. The Marquis de Guilhem-Clermont-Lodève, the Chevalier de Boufflers, Bishop Talleyrand, the bishop of Nancy, the archbishop of Aix, and numerous others made the transition with relative ease and were soon participating in the debates, some simply “bending to the circumstances,” as Boufflers described it, others with a real measure of idealism and enthusiasm, convinced that “the nobles . . . could be equally useful within the common hall of the Assembly.”⁵¹

It was not only these new recruits to the National Assembly who were affected. The entry of the privileged orders into their midst also had a profound effect on the deputies of the Third Estate. Numerous letters give expression to the explosion of joy and the feelings of fraternity with which the Third progressively welcomed the new arrivals. Whatever their rhetoric in late May and early June against the “aristocrats,” the majority of the commoners were still awed by the great nobles and flattered that they might sit with them in the same assembly. Adrien Duquesnoy was effusive with praise for “the finest names in the kingdom,” “the most virtuous men in the kingdom,” who now gave the Assembly “an aura of seriousness . . . that was previously lacking.” François-René-Pierre Ménard de la Groye, who had felt almost ashamed of his growing anger with the aristocrats in early June, again felt at ease when he dined with them and was

⁴⁸ Visme, “Journal des Etats généraux,” August 10, 1789; see also Dominique-Georges-Frédéric du Four de Pradt, “Quelques lettres de l’Abbé de Pradt, 1789–92,” ed. Michel Leymarie, *Revue de la Haute-Auvergne*, 56^e année, 34 (1954): introduction, 89–91; and Charles-Elie de Ferrières, *Correspondance inédite*, ed. Henri Carré (Paris, 1932), 82, 87, 108.

⁴⁹ La Gallissonnière, ms. journal, A4 LV1, folio 154, Archives de la Guerre.

⁵⁰ Pinteville, June 27, 1789, J 2286, Archives Départementales de la Marne.

⁵¹ La Gallissonnière, ms. journal, June 27, 1789, A4 LV1, Archives de la Guerre; also Ferrières, *Correspondance*, July 3, 1789; Guilhem-Clermont-Lodève, August 2, 1789, AA 23, Archives Communales d’Arles. Also Bernard de Brye, *Un évêque d’ancien régime à l’épreuve de la Révolution: Le cardinal A. L. H. de La Fare (1752–1829)* (Paris, 1985), 249–59; Charles-Maurice de Talleyrand, *Mémoires*, ed. Duc de Broglie, 5 vols. (Paris, 1891–92), 1:123–24; Eugène Lavaquerry, *Le Cardinal de Boisgelin, 1732–1804*, 2 vols. (Paris, 1920), 2:13–15.

delighted to observe “a great deal of unity and cordiality among all the members of the Assembly.”⁵²

Members of the Third Estate clearly went out of their way to encourage the participation of their new colleagues. The Vicomte de Malartic noted with evident satisfaction the deferential efforts of the commoners in his *bureau* to elicit his opinions on Saint-Domingue, where he had once lived.⁵³ Félix Faulcon waxed poetic as he described the camaraderie of nobles and commoners spending the night together in the assembly hall during the mid-July crisis: “These proud nobles, who once so greatly profited from their alleged privileges and the chance occurrence of their birth, now sleep or walk side by side with the commoners.”⁵⁴ The astute Ferrières rapidly seized up the new situation. “The Upper Third will be flattered,” he wrote his sister, “by the consideration shown them by the Nobles . . . Let the Nobles take a single step and the Third will take ten.”⁵⁵ Despite their suspicions of the “aristocrats” and the latter’s pretensions of social superiority and political dominance, the Commoners’ ultimate desire at this stage in events was not to destroy the Nobles but to be treated by that body as equals. Indeed, this surge of fraternal sentiments among the orders should not be underestimated in evaluating the night of August 4, the dramatic session during which substantial portions of Old Regime institutions were swept away in the space of a few hours. Even though most interpretations of the event emphasize the behind-the-scenes manipulations of the Breton Club, it is doubtful that such tactics would have been effective without the short-lived atmosphere of brotherhood that permeated much of the Assembly in early August.⁵⁶

Only a few of the Third Estate deputies, several of them future Jacobins, seem to have viewed the situation with a more cynical eye. The Breton deputy Delaville Le Roulx despaired that the Assembly would be “captivated by the seductive manners” of the Nobles and bishops and wondered how the Patriots might “bring fresh energy to the Assembly and prevent it from slipping into error.” Maillot noted that the “flattery and familiarity” of the privileged were more dangerous than their previous “arrogance and pride.” And Jacques-Antoine Creuzé-Latouche, who had never wanted a union of the three orders in the first place, and who would have preferred that the Third Estate act on its own to write a constitution, saw only divisions ahead in the Assembly. “Feeble

⁵² Duquesnoy, *Journal*, June 26, 1789; Ménard de la Groye, July 7, 1789, 10 J 122, AD, Sarthe.

⁵³ Malartic, “*Journal*,” July 7, 1789, MS. 21, BM, La Rochelle. Note also Visme’s entry of July 6: “j’ai vu avec plaisir . . . que les idées d’un membre de la noblesse n’aient point été négligées”; “*Journal des États généraux*.”

⁵⁴ Faulcon, *Correspondance*, journal entry of 3 a.m., July 15, 1789, p. 69.

⁵⁵ Ferrières, *Correspondance*, August 10, 1789. Compare the analysis of Talleyrand, *Mémoires*, 1:124.

⁵⁶ Numerous deputies commented on the unity and concord in the Assembly in the days before the event; see Maillot, August 1, 1789, JJ 7, AC, Toul; Charles-François Bouche to the Commissaires de communautés de Provence, August 2, 1789, C 1046, Archives Départementales des Bouches-du-Rhône; Charles Francville to the Municipality of Ardres, August 3, 1789, in François de Saint-Just, *Chronique intime des Garnier d’Ardres* (Paris, 1973), 118. Newspaper accounts of the period also placed enormous emphasis on the intense “désir d’union, . . . de la ‘fraternité’ retrouvée dans l’élan du cœur” just prior to August 4; Rétat, “Partis et factions en 1789,” 76. This is in no way to underestimate the influence of the Great Fear and the general anarchy of late July 1789 in the psychology of the deputies. See also Fitzsimmons, “Privilege and the Polity in France,” 286–91.

individuals," he wrote, previously brought into conformity with correct principles by the "vigorous and virile" in the Assembly would now be won over by the nobles, and "aristocratic and antipatriotic maxims" would become the order of the day.⁵⁷

Creuzé's fears were probably not unfounded. After the heroic days of the early revolution, there is considerable evidence that the Breton Club's influence went into decline. Whether or not the number of adherents actually diminished, the entry of 600 new deputies into the National Assembly—most of whom were clearly conservatives or moderates⁵⁸—invariably decreased the proportionate size of the radical contingent and reduced their hold on the large bloc of moderate Patriots within the Third Estate. In fact, by September, the "comité breton" seems to have reverted to an exclusively provincial organization, no longer attended by deputies from other provinces. It was apparently badly divided over the issue of the royal veto and was meeting less frequently than before.⁵⁹ Undoubtedly, the Patriots continued to meet to plan strategy in one way or another, but the meetings probably took place outside any formal organization. The Provencal deputy, Jacques-Athanase de Lombard-Taradeau, who openly aligned himself with the most advanced faction of the Patriots, never referred to the "Breton Club" after July but only to "what we call the 'Palais royal' of the Assembly" or simply "our party." And his descriptions of the group's operations portray an extremely loose factional organization improvised on the spot, "in the morning, before the beginning of the session, after much discussion among groups in the hall." Indeed, if Lombard's accounts are at all typical, it seems likely that the lengthy hours passed in the National Assembly and in the various discussion groups and committees throughout the months of August and September, the sheer fatigue from the work involved, made nightly club meetings substantially more difficult.⁶⁰

One indication of the decline in the Breton group's fortunes and the general movement of deputy opinion during this period comes—in the absence of roll-call records—from the various elections of officers to the National Assembly. The organizational regulations of the Assembly specified that every two weeks the deputies would meet in their *bureaux*—the thirty discussion groups, to which all members were assigned—to vote for a president and three secretaries. The

⁵⁷ Delaville Le Roulx, July 29, 1789, BB 12, AC, Lorient; Maillot, August 1, 1789, JJ 7, AC, Toul; Creuzé-Latouche, *Journal des Etats généraux*, 165–66. See also Durand, June 29, 1789, carton 5–56, Archives diocésaines de Cahors.

⁵⁸ See the discussion of "party" composition below.

⁵⁹ Bouchard, *Le club breton*, 90–92. On approximately September 2, Boullé, deputy from Pontivy, mentioned a special request by the Rennes representatives that the provincial delegation meet again to discuss a petition from the town of Rennes. The other Breton deputies apparently rejected the request; Boullé, *Revue de la Révolution*, 15 (1889), 117 (the letter is incorrectly dated September 28 by the editor). Delaville Le Roulx, September 18, 1789, BB 13, AC, Lorient, wrote that the Breton delegation had "de nouveau" opened its doors to deputies of other delegations as "avant et après le 17 juin," clearly suggesting that the doors had previously been closed. But there is no indication in Delaville's later letters that anything came of this initiative. Neither Walter, *Histoire des Jacobins*, nor Aulard, *Le Société des Jacobins*, mention the Breton Club between August 4 and late November or early December.

⁶⁰ Jacques-Athanase de Lombard-Taradeau, "Lettres (1789–91)," ed. L. Honoré, *Le Var historique et géographique*, 2 (1925–27): 245, 247, 261, 274–75, 324.

president would serve for a two-week period and would be chosen in multiple ballots if necessary—in an effort to obtain a winner by absolute majority; the secretaries would be chosen by simple plurality on a single vote and would hold office for a month (so that there were always six secretaries in service at a given moment).⁶¹ Although a great deal depended on the personalities of the individuals holding the posts, all the officers had considerable potential power: the president to set the order of debate and designate—or reject—speakers, the secretaries to control the minutes of the meetings and to sort correspondence and decide which letters and petitions went to which committees. Since the votes were organized through the thirty individual *bureaux*—in isolation from the pressures of the galleries—and were apparently taken by secret ballot, they can be interpreted as a useful index of the evolution of deputy sentiment and perhaps also of the degree of organization of the various political factions.⁶²

The earliest elections seem generally to confirm the atmosphere of a united front previously identified for this period. To be sure, with the possible exception of the Abbé de Montesquiou, Agent-General of the Clergy, all fourteen of the individuals elected as officers between July 3 and August 3 had earlier reputations as Patriots.⁶³ But while four of these—Le Chapelier, Sieyès, the Abbé Henri Grégoire, and Jérôme Pétion de Villeneuve—were probably considered radicals and later became members of the Jacobin Club, six others would undoubtedly have been classed as moderates, and four—Clermont-Tonnerre, Mounier, the Comte de Lally-Tollendal, and the Abbé de Montesquiou—soon embraced the more conservative “Monarchiens” position.⁶⁴ Perhaps equally significant, no less than ten of the fourteen were members of the privileged orders—though not all had actually been elected by their “natural” estates. This marked preference for nobles and clergymen as assembly officers continued throughout the entire first year of the Constituent Assembly with nineteen of twenty-seven presidents and fifty-one of eighty-eight secretaries being drawn from members of the first two estates.⁶⁵ The first three presidential contests were scarcely contests at all, with votes going overwhelmingly to the Duc d’Orléans (who declined), the archbishop of Vienne, and the Duc de Liancourt. The first and only Breton Club president, the Rennes lawyer Le Chapelier,

⁶¹ See the *règlement* of July 29, 1789; *AP*, 8:300–03.

⁶² *AP*, 33:88–91, lists the winners, but it is not entirely complete. To complete the list and to locate information about votes cast and opposition candidates, one must consult a wide range of sources, notably the minutes of the meetings in the *AP*, the *Moniteur*, and the official *Procès-verbaux*, as well as various newspaper accounts and deputy memoirs and letters. There were apparently sixty-three presidential elections, if one counts those replacing presidents who refused their elections or who resigned immediately. The generally perceptive deputy Duquesnoy was convinced that the votes for president mirrored his colleagues’ political affiliations at a given point in time; Duquesnoy, *Journal*, 2:127.

⁶³ Note that several of the fourteen were chosen more than once. Lafayette is included for his election as vice-president on July 13—the only time such an office was filled.

⁶⁴ On Monarchien and Jacobin membership, see below.

⁶⁵ This deference shown toward the privileged classes was equally in evidence in the choice of presidents for the thirty *bureaux* elected at intervals during the summer: at least twenty-seven of the thirty in early July and fifteen of sixteen for which data exist in mid-September were either Nobles or bishops; *AP*, 8:185; and C 83, dossier 818 (14), Archives Nationales (hereafter, AN). Note that the family of Le Chapelier was newly ennobled. Sieyès was, of course, a clergyman.

obtained the post almost by accident in early August. In fact, he had come in third in the initial contest and was chosen in a second election only after the victor, the moderate Jacques-Guillaume Thouret, had resigned.⁶⁶ And it was to be the last time a member of the Left would control the rostrum for almost seven months. Through the end of the year, the future Jacobins were largely an insignificant force in the presidential tallies, their candidates unable to muster more than 183 votes (out of over a thousand) in any of the elections for which voting totals are preserved.⁶⁷ Indeed, to judge by the deputies elected to office from mid-August to mid-October, the best organized and most influential faction within the Assembly was not on the Left at all but on the Right.⁶⁸

THROUGHOUT THE MONTH OF JULY, the organization of the recalcitrant privileged faction seems never entirely to have dissolved, despite the popular upheavals and the temporary flight of many of its adherents. Even after the meeting halls of the Nobles and the Clergy had been closed and converted into offices by Jacques Necker, director-general of finance, a core of the most conservative noblemen and bishops continued to meet in the homes of individuals. They were clearly acting as a corps on July 16 when they announced to the Assembly that they would henceforth join in the votes and the debates.⁶⁹ It was most likely this same coalition that then coordinated the considerable deputy discipline involved in placing several of its numbers on the new Committee on Research at the end of July and in the election of Thouret to the presidency at the beginning of August.⁷⁰ Significantly, it was toward the beginning of August that several Patriots first took note of the “cabal” of nobles and clergy that was opposing them and voting as a bloc.⁷¹

⁶⁶ On the election of August 1–3, see Boullé, *Revue de la Révolution*, 15 (1889), 101; Gaultier de Biauzat, *Correspondance*, 2:221; Ernest Lebègue, *La vie et l'oeuvre d'un constituant: Thouret* (Paris, 1910), 142–43; Paul Bastid, *Sieyès et sa pensée* (Paris, 1939), 78.

⁶⁷ Pétion received 183 votes on September 12 and 143 on September 28—although some of the votes of the Left may have gone to Jacques-Guillaume Target, who received thirty-seven and fifty-two votes, respectively. On November 23, the Duc d'Aiguillon received 166; AN, C 83, dossier 818 (1–3); Comte de Virieu to the Marquis de Viennois, September 29, 1789. Archives of the Château de Viennois, from a copy kindly loaned to me by Jean-Louis Flandrin; and Daniel Ligou, *La première année de la Révolution vue par un témoin* (Paris, 1961), 169.

⁶⁸ On the general problem of the Right in the early revolution, see Paul Beik, *The French Revolution Seen from the Right in Transactions of the American Philosophical Society*, 46 (1956), part 1; Philip Kolody, “The Right in the French National Assembly, 1789–91” (Ph.D. dissertation, Princeton University, 1967); Jean-Paul Bertaud, *Les Amis du roi: Journaux et journalistes royalistes en France de 1789 à 1792* (Paris, 1984); William James Murray, *The Right-Wing Press in the French Revolution, 1789–1792* (London, 1986).

⁶⁹ La Gallissonnière, undated entry, ms. journal, A4 LVI, folio 155, Archives de la Guerre. Several deputies noted the presence of Eprêmesnil—one of the key leaders of the reactionary group—among the nobles making the announcement that day; see Delaville Le Roulx, July 17, 1789, BB 12, AC, Lorient.

⁷⁰ Jacques Jallet, *Journal inédit*, ed. J.-J. Brethé (Fontenay-le-Comte, 1871), July 28, 1789, was impressed by the unusually large number of “aristocrats” who appeared on July 28 for the committee elections. See also Maillot, August 1, 1789, JJ 7, AC, Toul.

⁷¹ Maillot, August 1, 1789, JJ 7, AC, Toul; Delaville Le Roulx, August 4, 1789, BB 12, AC, Lorient; Lombard-Taradeau, “Lettres (1789–91),” August 5, 1789; Boullé, *Revue de la Révolution*, August 14, 1789.

An initial turning point in the new evolution was undoubtedly the night of August 4. While in some respects this sweeping attack on privilege marked the ultimate fruition of the earlier flowering of fraternal generosity, it also carried with it the seeds of renewed factional strife. To judge by the reflections of the deputy letter writers, the suppression of seigneurial rights was accepted with resignation and sometimes with enthusiasm. "If it leads to advantages for the public good," wrote Ferrières to his wife, "I will easily be able to console myself for my losses as a noble and as a seigneurial lord." The chevalier Garron de la Bévière was more morose about the economic prospects of his order, but he, too, in a letter to his wife, accepted the inevitable: "In the end, if it will promote general happiness, I have no regrets . . . One must yield to necessity."⁷² Yet large numbers of the clerical deputies were clearly upset by the suppression of the tithes without reimbursement and by the first proposals for the nationalization of church property. According to numerous witnesses, however, the key issue that united many of the nobles and clergymen as a solid and cohesive group was the debate two weeks later over including religious toleration in the Declaration of the Rights of Man and the Citizen and the counterproposal that Catholicism be made the state religion. In what were widely described as the most tumultuous debates to date, a number of deputies in all three estates seem to have dramatically crystallized their opposition to the revolution.⁷³ Thus curé Emmanuel Barbotin, previously a strong supporter of the Third Estate, now became convinced that many Third Estate deputies were "philosophers who have neither faith nor discipline"; while Guillaume Gontier de Biran, a chief *bailliage* magistrate from Bergerac, first came to perceive a dual menace to religion and the throne. For the Baron de Gauville—who had been irritated by the loss of his hunting rights on August 4 but who had generally accepted the abolition of seigneurial dues—it was precisely during these debates of late August that "we began to recognize one another" and that he and his colleagues began sitting together consistently on the right side of the president's table.⁷⁴

Nevertheless, the critical achievement in the organization of the Right was to be the work of a new coalition of more moderate conservatives, a number of them recruited from the Third Estate. The formation and general character of the "Monarchiens" have been described in some detail by Jean Egret and, more

⁷² Ferrières, *Correspondance*, August 6, 1789; Claude-Jean-Baptiste Garron de la Bévière to his wife, August 5, 1789, 1 Mi 1, Archives Départementales de l'Ain.

⁷³ On the violence and intensity of the debates in mid-August, see Boullé, *Revue de la Révolution*, 14 (1889), 104; and Maupetit, "Lettres," 19 (1903): 226. The original motion of August 4 had called for suppression of the tithes with some form of reimbursement, but this was changed in the days that followed. The earliest motions for the expropriation of church lands had been made on August 8 by the Marquis de Lacoste and Alexandre Lameth.

⁷⁴ Guillaume Gontier de Biran to the Municipality of Bergerac, retrospective letter of May 22, 1790, carton 1, Archives Communales de Bergerac, Fonds Faugère; Emmanuel Barbotin, *Lettres de l'abbé Barbotin*, ed. A. Aulard (Paris, 1910), August 23 and 29, 1789; Gauville, *Journal*, 16–20. A similar picture is confirmed by the patriots Lombard-Taradeau, "Lettres (1789–91)," 263; Gaultier de Biauzat, *Correspondance*, 2:269–70; and François-Antoine Boissy d'Anglas, "Lettres inédites sur la Révolution française," ed. René Puaux, *Bulletin de la Société de l'histoire du Protestantisme français*, 75 (1926): 433. It was also in August that the newspapers first began mentioning "cabals" and "coalitions" in the Assembly; Rétat, "Partis et factions en 1789," 77.

recently, by Robert Griffiths.⁷⁵ Unlike the extreme right of the recalcitrant Nobles and Clergy, who sought either a return to the Old Regime or a system of reforms based on the king's declaration of June 23, the Monarchiens sought to affirm the transformations of that summer but to ensure that ultimate sovereignty remained in the hands of the king as a buttress against the dangers of popular violence. Centered on the delegation from Dauphiné—the only provincial delegation that could match the Bretons in its cohesiveness and its tradition of group action—but also including important contingents from Auvergne and Normandy and a bevy of moderate nobles, the group resolved sometime in late July or early August to beat the Breton Club at its own game.⁷⁶ In relatively short order, the Monarchiens had surpassed the Patriots in their level of factional organization. While the Breton Club had operated in an essentially democratic fashion, with relatively loose discipline and public debates in a café to which all were invited, the Monarchiens followed their more authoritarian and hierarchical penchant by establishing a small decision-making “central committee,” which convened in private at one of the member's homes—and sometimes in the château of Versailles itself—and which then sent out directives through a system of subcommittees to all its potential adherents. Before votes in the *bureaux* for committee members or National Assembly officers, someone passed out notes listing the names that deputies were to inscribe on their ballots.⁷⁷ On the floor of the National Assembly, Mounier's friend and colleague from Dauphiné, the Comte de Virieu, assumed the role of a veritable party whip: “he can be seen in every corner of the hall, speaking, entreating, shouting, peering to see who will vote for or against.”⁷⁸ By early September, the progressively tighter coalition between moderate and extreme right was clearly in place, with Cazalès, Epréménail, and the Abbé Maury participating in the Monarchien central committee and speaking frequently in the Assembly in defense of Monarchien positions.⁷⁹ On September 17, the radical printer-deputy from Lyon, Jean-André Périsset-Duluc, wrote that “the coalition of nearly all of the Clergy and the Nobles, along with a lesser number of Commoners, has become so strong that the deputies involved never differ on their votes: without exception, all of them rise together

⁷⁵ Egret, *La révolution des notables*; Robert Griffiths, *Le centre perdu: Malouet et les “monarchiens” dans la Révolution française* (Grenoble, 1988). Griffiths placed the Monarchiens in the “center.” However, the group is known to have allied itself with the extreme right and, on most issues, to have voted to the “right” of the large group of unaligned deputies. I would prefer to use the term “center” for the latter group.

⁷⁶ Jean-André Périsset-Duluc to J. B. Willermoz, September 17, 1789, MS. 5430, Bibliothèque Municipale de Lyon (hereafter, BM, Lyon), claimed the Normans led by Thouret were initially part of the coalition. There were at least seven deputies from Auvergne, led by Malouet; Egret, *La révolution des notables*, 126–28; Griffiths, *Le centre perdu*, 109–10.

⁷⁷ Montlosier, *Mémoires*, 1:277; Malouet, *Mémoires*, 1:301–02. Gaultier de Biauzat also observed this; *Correspondance*, 2:269–70.

⁷⁸ Périsset-Duluc, September 17, 1789, MS. 5430, BM, Lyon. Virieu himself described his role in similar terms: “je me suis promis de faire toutes les avant gardes dangereuses et difficiles dont d'autres ne se seront pas chargés”; Virieu to the Marquis de Viennois, August 25, 1789, Archives of the Château de Viennois.

⁷⁹ Montlosier, *Mémoires*, 1:277; Périsset-Duluc, September 17, 1789, MS. 5430, BM, Lyon; Albert Mathiez, “Etudes critiques sur les journées des 5 et 6 octobre 1789,” *Revue historique*, 67 (1898): 266, 273. On the general question of an alliance between the extreme right and the Monarchiens, see especially Kolody, “The Right in the French National Assembly,” 122–34.

or remain seated [in order to vote].”⁸⁰ Clearly, many of the “aristocrats” were increasingly prepared to follow the rules of the game and work within the newly evolved parliamentary system, convinced of the real possibility of halting and perhaps reversing the revolution through political organization and majority votes.

The growing power of this new coalition was apparent in the choice of Assembly officers. After the middle of August, the Monarchiens not only won four successive presidential elections but also largely dominated the secretariat’s table as well. In the election of August 31, they even obtained a clean sweep of the president—the bishop of Langres—and all three secretaries. Indeed, there is good evidence that the Monarchien coalition was the only group systematically organizing for elections during this period. The scraps of voting records remaining reveal that, in September, the Monarchien candidates alone received significant blocs of votes in every *bureau*, the remainder of the votes being spread out over an enormous range of individual deputies.⁸¹ “For three weeks now,” wrote Virieu after the election of the bishop of Langres, “the reasonable and upright deputies have quietly reclaimed the majority. The *enragés* have been beaten back on all fronts. In spite of their efforts, we have chosen the president and all three secretaries.”⁸² The election in mid-September of the Monarchien Comte de Clermont-Tonnerre was perhaps even more galling to the Left in that, by the rotation procedure tacitly agreed on since July, the post should normally have gone to a member of the Third Estate.⁸³

To be sure, the coalition was frequently more successful in the electoral *bureaux* than on the floor of the Assembly itself, and it came up short on several of the key constitutional votes for which it militated with particular fervor—above all, the effort in early September to obtain a two-house legislature and an absolute royal veto on all legislation. Indeed, the alliance would seem to have broken down entirely on the issue of the number of chambers, with the extreme right apparently following a *politique du pire* and voting with the Patriots. But most of the deputies perceived the veto decision as a compromise vote between the Right’s desire for an absolute veto and the Left’s desire for no veto at all. And the Monarchiens and their allies won a considerable victory later in the month when it was decided that three successive legislatures would have to pass the

⁸⁰ Périsse-Duluc, September 17, 1789, MS. 5430, BM, Lyon.

⁸¹ Thus, in the secretarial election of August 29, 1789, only the three Monarchien candidates, Claude Redon, Pierre-Suzanne Deschamps, and Jean-Louis Henry de Longuève, obtained large blocs of votes; the remainder of the votes apparently represented individual, uninstructed choices; AN, C 83, dossier 818 (6–7).

⁸² Virieu, September 1, 1789, Archives of the Château de Viennois. See also the analysis of Théodore Vernier to the Municipality of Lons-le-Saunier, August 30, 1789, “Lettres de Vernier,” Archives Communales de Bletterans (non-classé).

⁸³ The rotation system, arranged by verbal agreement, was described by Bouche, August 31, 1789, C 1046, Archives Départementales des Bouches-du-Rhône. He also noted his anticipation that the mid-September election would go to a commoner. See also La Gallissonnière, undated entry, ms. journal, A4 LVI, folio 153, Archives de la Guerre. It is clear that most of the deputies were still voting by the rotation system in the previous election: 802 out of 834 votes were cast for clergymen; AN, C 83 (1–3).

same law in order to override a royal veto—a complication considered by some as tantamount to an absolute veto.⁸⁴

In any event, the deputies of the Left clearly believed themselves under siege from late August to early October. Lombard wrote home that “our party is absolutely in the minority.” Louis-Prosper Lofficiel was convinced that, without the support of about forty clergymen and a hundred or so liberal nobles, “we would certainly be defeated” on every vote. Périsset estimated that as many as two-thirds of all the deputies were influenced by the “cabal”—although fortunately half of these were open-minded and could sometimes be won over.⁸⁵ For the celebrated writer, Constantin-François Chassebeuf de Volney, deputy from Anjou, the Assembly was now so divided and in danger of being won over by the aristocrats that it was necessary to elect a whole new assembly and a new set of deputies, chosen this time to represent the true social composition of the French population—notably, by eliminating most of the noble and clerical deputies. And after losing the vote on the manner of overriding a royal veto, the delegation from Brittany seriously discussed abandoning the Assembly altogether, an Assembly now deemed to be entirely dominated by the “aristocrats.”⁸⁶

THE POLITICAL HISTORY OF THE CONSTITUENT ASSEMBLY after the dramatic events of October 5–6—the march on Versailles that led to the transfer of both monarchy and Assembly to Paris—is generally less well known than the earlier period of the revolution. Many series of correspondence ended in the later half of 1789 or toward the beginning of the following year, as deputies found themselves burdened with ever-increasing demands on their time and as a growing national distribution of newspapers removed one of the principal *raisons d'être* for the letters home.

Nevertheless, it seems clear that the October Days did not mark the demise of the Right as a meaningful force in the National Assembly. The mass desertion of the conservative deputies—300 are sometimes said to have taken out passports⁸⁷—is apparently a myth, altogether unsubstantiated by the evidence

⁸⁴ Delaville Le Roulx, September 22, 1789, BB 13, AC, Lorient; and Jean-François-Marie Goupilleau to his cousin, senechal in Rochefevrière, undated letter of late September, Collection Dugast-Matifeux, no. 98, Bibliothèque Municipale de Nantes.

⁸⁵ Lombard-Taradeau, “Lettres (1789–91),” 271; Louis-Prosper Lofficiel, “Lettres de Lofficiel,” ed. M. Leroux-Cesbron, *La nouvelle revue rétrospective*, 7 (1897): 111; Périsset-Duluc, September 2, 1789, MS 5430, BM, Lyon. Note also Durand: “il y a dans l'Assemblée une telle division . . . qu'il est encore bien problématique lequel aura le dessus”; September 5, 1789, carton 5–56, Archives diocésaines de Cahors. Maillot spoke of the patriots as “le parti de la minorité”; August 30, 1789, JJ 7, AC, Toul. See also the letter of Robespierre, *Correspondance*, 51; and of Goupilleau, undated, ca. late September, Collection Dugast-Matifeux, no. 98, BM, Nantes.

⁸⁶ On the Volney motion, see Lofficiel, “Letters,” September 14 and 18, 1789, pp. 111–13; and Visme, “Journal des Etats généraux,” September 18, 1789. On the Breton discussion, Delaville Le Roulx, September 22, 1789, BB 13, AC, Lorient; and Barbotin, *Lettres*, 61.

⁸⁷ Apparently first reported by Hippolyte Taine and followed by numerous other historians. See Eric Thompson, *Popular Sovereignty and the French Constituent Assembly, 1789–91* (Manchester, 1952), 24.

available.⁸⁸ Although four of the Monarchien leaders did indeed leave the Assembly,⁸⁹ Pierre-Victor Malouet, Clermont-Tonnerre, Virieu, and their associates continued their efforts on behalf of the Monarchien platform to the very end of the Constituent Assembly. Together, they formed first a “Club des Impartiaux” and later a “Club Monarchique.”⁹⁰ Despite his initial despondency over the October events, Virieu ultimately repudiated the desertion of his friend Mounier and vowed to fight on: “the sacred flame that burns within me is not yet extinguished, and is reviving . . .” “I will stay with the Assembly,” wrote Archbishop Boisgelin, “and I will go with it [to Paris].” Gontier de Biran also affirmed his determination to stay on despite his anguish and disappointment over the events: “I think that if it were not for the honor and desire of doing our duty, few of us would remain here.”⁹¹

For the next few months, the focus of power seemed to shift back toward the center of the political spectrum, with the unaligned “Center” Patriots—Emmanuel-Marie Fréteau de Saint-Just, Armand-Gaston Camus, Thouret, and Jean-Nicolas Dêmeunier—picking up five of the six presidencies to the end of the year.⁹² Yet the factional organization, the very considerable coordinating capabilities created by the Monarchiens, seem to have been maintained. On occasion, the group was still able to elect its candidates as president: Boisgelin in November and the Abbé de Montesquiou in early January.⁹³ Basking in his recent victory, the prelate congratulated himself for having rejected all the predictions of doom by Mounier and Lally-Tollendal and for having stayed on to fight. Where would he be now, he mused, “if I had listened to the advice everyone was giving me?”⁹⁴ Like many other deputies, he was convinced that the decree of November 2 placing ecclesiastical lands “at the disposal of the Nation” was actually a victory for his side. Everyone knew that some church lands would have to be sold, he wrote, but they had succeeded in simply admitting the principle without in any way turning all lands over to the state: “They will perhaps be satisfied to sell off monastic property.”⁹⁵ The Monarchiens and their

⁸⁸ Malouet wrote that the Monarchien leadership hoped for 300 resignations; perhaps this is the origin of the myth. But he admitted that only twenty-six deputies actually requested passports; *Mémoires*, 2:4–5. This is approximately confirmed by the records of the National Assembly itself; AN, C 32, dossier 266. Mirabeau spoke of 300 requests on October 9, but this was probably an exaggeration; *AP*, 9:389.

⁸⁹ Mounier and Lally-Tollendal resigned almost immediately; Bishop La Luzerne resigned within a month; Bergasse abandoned the Assembly without ever formally resigning.

⁹⁰ In early January 1790, the group met with Lafayette, La Rochefoucauld, and a faction of the moderate patriots—and perhaps secretly with Mirabeau—and almost succeeded in engineering a new “coalition” on the center right. See Malouet, *Mémoires*, 2:45–48; Montlosier, *Mémoires*, 2:35; *Mercur de France*, January 1790, p. 164.

⁹¹ Virieu, October 12 and 16, 1789 Archives of the Château de Viennois; Jean-de-Dieu Boisgelin de Cucé to Comtesse de Gramont, October 6, 1789, AN, M 788; Gontier de Biran, October 12, 1789, carton 1, Archives Communales de Bergerac, Fond Faugère.

⁹² None of the four would be members of the Jacobin Club during the period of the Constituent Assembly or sign the petition of the “Capuchin” group; see below.

⁹³ Note also that, in the election of November 13, Boisgelin came within seven votes of a victory on the second round; Boisgelin de Cucé, undated, ca. November 13, 1789, *pièce* 136, AN, M 788.

⁹⁴ Boisgelin de Cucé, undated, ca. mid-November 1789, *pièce* 141, AN, M 788.

⁹⁵ Boisgelin de Cucé, November 3, 1789; also November 7 and 23, AN, M 788. Similar views were also expressed by the moderates; Maupetit, “Lettres,” 19 (1903): 371; Visme, “Journal des Etats généraux,” November 2, 1789; André-Marie Merle to the Municipality of Mâcon, November 4,

allies also continued to obtain election of their adherents to various committees, culminating in a dramatic vote in late November that gave them effective control of the powerful organ of investigation and repression, the Committee on Research.⁹⁶

Yet the October Days may well have marked the beginning of a certain shift within the Right coalition in favor of the more reactionary elements. This evolution is particularly evident if one examines the breakdown of those deputies actually participating in Assembly debates—as suggested by entries in the index to the proceedings and debates of the Constituent Assembly. (See the graph.⁹⁷) The frequency of participation of the Monarchiens dropped precipitously from September to March, continuing downward even after the departure of the four Monarchien leaders in October. During the same period, the most notable speakers of the extreme right were participating ever more frequently, so that from early 1790 to the end of the Constituent Assembly they had become the principal spokesmen for the Right. Unfortunately, much less is known of the factional organization of the extreme right during this period. But the religious issue, and particularly the question of church property seems increasingly to have become the central rallying point in binding together the most conservative elements of the Nobles, the Clergy, and the Commoners—or such, at least, was the opinion of the conservative Baron de Gauville.⁹⁸

Significantly—in the midst of the debates over ecclesiastical land and monastic vows—the group began holding its meetings in the Grands Augustins, in the same hall where the bishops had been accustomed to holding their *Assemblées générales* for generations. Here, according to the elderly Patriot lawyer René-Antoine-Hyacinthe Thibaudeau, “They begin by preparing in their committees the insidious measures that they then try to push through the Assembly.”⁹⁹ When the faction was forced to leave under the pressures of the Parisian crowds,

1789, D2 no. 13 (carton 21 bis), Archives Communales de Mâcon; Jean-François Begouen-Demeaux to municipal officers of Le Havre, October 31, 1789, D (3) 38–39, Archives Communales du Havre; and by the future Jacobin, Vernier, Archives Communales de Bletterans, November 3, 1789. Not all deputies agreed, however; see, for example, Robespierre, *Correspondance*, 57; and Goupilleau, November 2, 1789, Collection Dugast-Matifeux, no. 98, BM, Nantes.

⁹⁶ Visme, “Journal des Etats généraux,” November 21, 1789. Seven of the twelve members elected were future “Capuchins” (see below); AN, C 32, dossier 274.

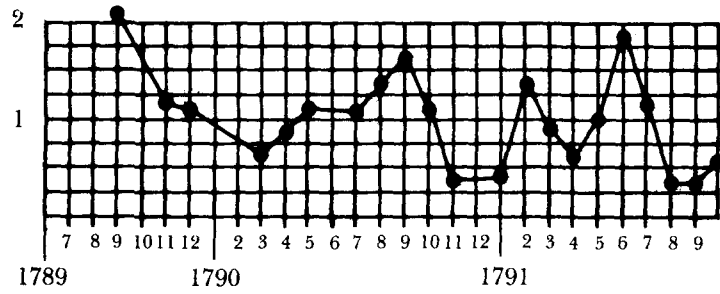
⁹⁷ I have used vol. 33 of the *AP*, which is the index to vols. 8–32, dealing with the Constituent Assembly. Represented are the leading speakers of the Monarchiens and the extreme right as determined by the length of the entries for each deputy in the index: for the Monarchiens: Jean-Joseph Mounier, Comte de Lally-Tollendal, Nicolas Bergasse, Bishop La Luzerne, Comte de Virieu, Clermont-Tonnerre, Malouet, Pierre-Joseph de Lachèse, Amable-Gilbert Dufraisse-Duchey, and Noël-Joseph Madier de Montjau; for the extreme right: Abbé Maury, Cazalès, Eprémèsnil, Marquis de Foucauld Lardimalie, Marquis de Bonnav, Marquis de Folleville, Reynaud de Montlosier, and Vicomte de Mirabeau. Displayed on the graph is the average weekly frequency of participation plotted as an index around the overall mean for the entire period of the Constituent Assembly, where the mean is set at 1.0. Although the index does not give precise dates, these can be extrapolated from the volume numbers—which are given. The *Archives parlementaires* is undoubtedly the best single source for debates in the National Assembly, but unfortunately it does not include all speeches given in the Constituent Assembly, and approximately 5 to 10 percent of those that it does mention seem to be missing in the index—probably through the carelessness of the editors.

⁹⁸ Gauville, *Journal*, 59.

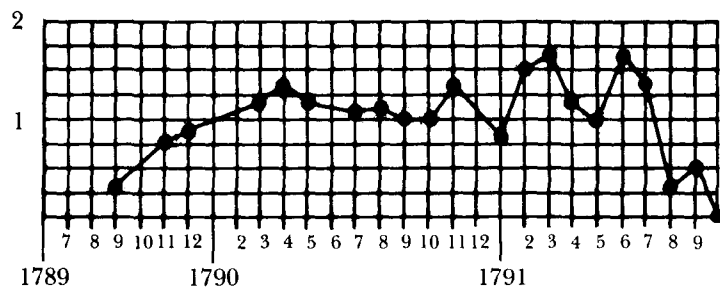
⁹⁹ Thibaudeau to Faulcon, undated but probably early January 1791; printed in Faulcon, *Correspondance*, 141.

Index of Participation

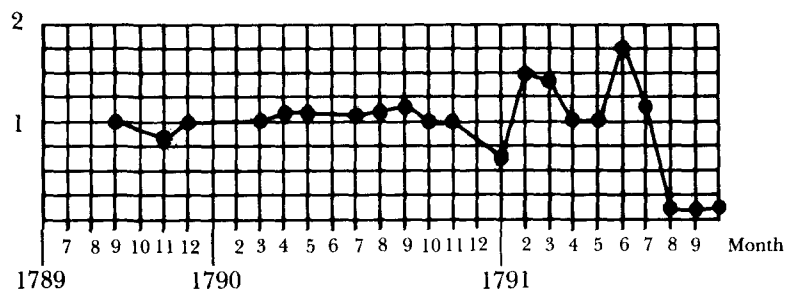
10
Monarchiens



8
Extreme
Right



18
Monarchiens
and
Extreme Right



Date

they took refuge in the Capuchin monastery, which was directly attached to the assembly hall and which allowed the deputies to enter through a private passageway unseen by the crowds outside.¹⁰⁰ It was here that the “Capuchin Society”—as they came to be known—drew up their declaration of April 19, 1790, adhering to Dom Christophe-Antoine Gerle’s motion six days earlier that Roman Catholicism be declared the state religion.¹⁰¹ Chased away once again by the Parisian crowds, the group seems to have continued its coordination in more secretive fashion through a series of small “committees” that met in the homes of individuals, each committee delegating one of its members as liaison with a central steering committee. On occasion, they seem also to have met jointly with the Club des Impartiaux—confirming a continued loose alliance between the two alignments on the Right.¹⁰²

By the spring of 1790, there were already numerous signs of a shift of the Assembly’s center of gravity in the direction of the Left. To believe the analysis of Visme, at the beginning of December the deputies were divided almost exactly into two equal parts. In the important vote of December 7, when the radicals attempted to revise the election laws to promote broader political participation, “the Assembly divided into two almost equal parts sitting at opposite ends of the hall.”¹⁰³ And Périsset-Duluc noted the hesitancy of the Left to have any of its adherents sent out as *commissaires du roi* to organize the new local governments, for fear that the absence of even a few Patriots would tip the balance in favor of the Right.¹⁰⁴ But, by late December and early January, a number of deputies from various points on the political spectrum were becoming aware of an evident erosion of the Right. By year’s end, the Protestant banker from Lyon, Guillaume-Benoît Couderc, was convinced that “the aristocratic influence in the hall is declining appreciably from day to day.” The Patriot curé Thomas Lindet concurred: “The opposition party is diminishing . . . It is winning some small victories, but it is losing the major questions.” And Delaville, so pessimistic for the Left the previous September, now suggested, for the first time, that his “party” was clearly in control of the situation: “les hommes forts,” as he described them. “Those who know anything of the Assembly,” wrote Duquesnoy, “cannot help but notice the progressive desertion and depopulation of that part of the hall where the Abbé Maury sits, and that there is no longer a sufficient number of seats at the other side of the hall.” By mid-January, the Assembly staff was having to install more benches on the Left to accommodate

¹⁰⁰ Lavaquery, *Le Cardinal de Boisgelin*, 2:74–76. Du Bus, *Stanislas de Clermont-Tonnerre*, 224, equated the Grands Augustins meetings with the Club des Impartiaux. This is probably not correct, although there was a substantial overlap between the two groups.

¹⁰¹ Thomas Lindet, *Correspondance de Thomas Lindet pendant la Constituante et la Législative* (1789–92), ed. A. Montier (Paris, 1889), 115–16; Lameth, *Histoire de l’Assemblée constituante*, 2:148–49; Gaultier de Biauzat, *Correspondance*, 2:307–12. Also, Louis-Jean-Baptiste Leclerc de Lassigny de Juigné to his wife, April 14, 1790, Archives of the Château de Saint-Martin (Taradeau, Var).

¹⁰² Lavaquery, *Le Cardinal de Boisgelin*, 2:74–76. The home of Eprémessnil seems to have been one of the central meeting places; Montlosier, *Mémoires*, 2:328, 334.

¹⁰³ Visme, December 7, 1789, “Journal des Etats généraux.” The vote was 453 for the Right and 443 for the Left. See AP, 10:414–15.

¹⁰⁴ Périsset-Duluc, December 27, 1789, MS. 5430, BM, Lyon.

all the new arrivals.¹⁰⁵ To be sure, the Right retained a considerable residual strength through at least the middle of 1790 and was able to attract enough votes from the still rather volatile uncommitted deputies to win several presidential elections and a certain number of constitutional votes. Indeed, from late February through the middle of July, the elections suggest an Assembly more polarized than ever before. One might be tempted to speak of a veritable two-party system during this period, with nine of the eleven presidential victories going to either the Jacobin or the Capuchin candidates.¹⁰⁶ But, by and large, the momentum and the initiative within the Assembly were increasingly passing to the left of hall.¹⁰⁷

The reasons for this evolution at this particular moment in the Assembly's history are not entirely clear. It was related in part, no doubt, to the increasing dominance within the Right of the most reactionary strand of conservatism, the strand associated in the minds of most deputies with the trio of Maury, Cazalès, and Eprémèsnil. Many of the Monarchiens had built earlier reputations as patriots and reformist leaders. But the leaders of the opposition most in evidence by the end of 1789 had been identified from the beginning with a complete return to the Old Regime. As Duquesnoy suggested, far fewer people wanted to sit on the Right when the Abbé Maury became the central figure on his side of the hall.¹⁰⁸ The situation was compounded in that the geography of the new meeting hall in Paris no longer provided any intermediate places in which to sit. The hall in Versailles had been essentially oval in its layout, while the long, narrow "Manège," divided in the middle by the speaker's platform and the president's table, forced every deputy to make a daily symbolic affirmation as to which side he was on.¹⁰⁹

Yet perhaps the single most important development in the resurgence of the Left was the formation in late November or early December of the Jacobin Club and the rapid emergence of this association as a highly organized political force. In fact, the new *société* was not—as is often suggested—the simple continuation of the Breton Club, transferred from Versailles to Paris. By the end of the summer, the Breton group had already lost its character as the central rallying point for all Patriots. Once in Paris, the Bretons apparently continued their separate meetings for a time even after the creation of the Friends of the

¹⁰⁵ Guillaume-Benoît Couderc, "Lettres de Guillaume-Benoît Couderc (1781–92)," ed. M. O. Monod, *Revue d'histoire de Lyon*, 420; Lindet, *Correspondance*, 38; Delaville Le Roulx, January 18, 1790, BB 13, AC, Lorient; Duquesnoy, *Journal*, 2:196–97, 269. See also Faulcon, *Correspondance*, 2:140–41; Ménard de la Groye, January 1, 1790, 10 J 122, AD, Sarthe; and Goupilleau, January 11, 1790, Collection Dugast-Matifeux, no. 98, BM, Nantes.

¹⁰⁶ The presidents on the Right: Abbé de Montesquiou, the Marquis de Bonnavay (twice), and the Comte de Virieu; on the Left: Rabaut-Saint-Etienne, Baron de Menou, Bon-Albert Briois de Beaumez, Abbé Sieyès, and Louis-Michel Le Pelletier de Saint-Fargeau.

¹⁰⁷ This was particularly the case in the series of major decrees concerning the reorganization of the church, culminating in the Civil Constitution of the Clergy. But the Left also won major victories in votes on the judicial system and the right to declare war.

¹⁰⁸ Duquesnoy, *Journal*, 2:269. See also Jean-François Campmas to his brother, vicaire in Carmaux, December 24, 1789, MS. 177, Bibliothèque Municipale d'Albi.

¹⁰⁹ Armand Brette, *Histoire des édifices où ont siégé les assemblées parlementaires de la Révolution* (Paris, 1902).

Constitution.¹¹⁰ Although a great many of the members of the new association had probably also been members of the earlier Breton Club, the Jacobins created a new kind of Patriot structure, more highly centralized and organized, patterned in many respects after the organization of the Right. Indeed, according to Louis-Marie de La Revellière-Lépeaux, the initial formation of the Jacobin Club in late November was in direct response to the organizational offensive of the Right. Everyone knew, wrote La Revellière, that “the aristocratic party normally chose the Assembly officers because it held meetings in which it was decided in advance who was to be elected.” For this reason, the Left “decided to hold meetings of their own so that they could ensure the Patriots’ control of the *bureau*.”¹¹¹

The details of this organization are still poorly known. It seems certain, however, that in addition to their general public meetings in the Dominican convent, the Jacobins created a central committee with prime responsibilities for guiding the general direction of the club and set up a far more efficient means of disciplining voting. But if, in many respects, they simply emulated the organization of the Monarchiens, the Jacobins also went beyond the Right in their efforts to systematically mobilize public opinion in favor of their initiatives through the creation of a correspondence committee as liaison with affiliated clubs in the provinces.¹¹² It was almost certainly this new organization that allowed the Jacobins to increase their influence in the election of Assembly officers—first, from November onward, the secretaries and, by March 1790, the presidents as well. The same organization enabled the Left to begin systematically taking control of most of the committees. Thus, in the December election of the Committee on Research, the club engineered a dramatic turnabout, with the elimination of all the deputies on the Right and their replacement by twelve known Patriots, eight of whom were Jacobins.¹¹³ While in 1789 they obtained only a fourth of the committee assignments, in 1790 the Jacobin group was able to gain half of all new positions. Over the same period, the deputies on the Right saw their share of committee posts decline from one-fifth to less than one-tenth of all assignments.¹¹⁴

¹¹⁰ Bouchard, *Le club breton*, 94; and Lemay, *La vie quotidienne*, 216. Delaville Le Roulx, November 30, 1789, BB 12, AC, Lorient, and others, mentions Breton Committee meetings in Paris discussing general subjects.

¹¹¹ Louis-Marie de La Revellière-Lépeaux, *Mémoires*, 3 vols. (Paris, 1895), 1:85.

¹¹² Walter, *Histoire des Jacobins*, 55. Grégoire describes the importance of mobilizing petitions from Jacobins throughout France in order to pressure the decisions of the National Assembly; *Mémoires*, 1:387. See also Michael L. Kennedy, *The Jacobin Clubs in the French Revolution: The First Years* (Princeton, N.J., 1982), esp. chap. 1.

¹¹³ AP, 32:564. At the last and final election of the Comité, in April 1790, nine of the twelve men elected—all Jacobins or known Patriots—received almost exactly the same number of votes—between 196 and 206, a sure sign of the collusion involved; AN, C 38, dossier 334.

¹¹⁴ In 1789, future Jacobins obtained 160 (25 percent) and future Capuchins obtained 118 (19 percent) of a total of 636 committee assignments; in the first six months of 1790, Jacobins obtained eighty-six (48 percent) and Capuchins twenty-one (12 percent) of a total of 180 new assignments. Falcon thought that it was impossible to be named to a committee in 1790 unless one belonged to a club; *Correspondance*, May 29, 1789, 236.

By the early weeks of 1790, both outside observers and the deputies themselves were aware of the growing polarization of the National Assembly and of the extent to which developments in that assembly were increasingly dominated by two politicized and well-organized "parties." As *L'Observateur* remarked, "For the last month, two associations have existed in Paris. Each is composed of members of the National Assembly. The first . . . meets in the Jacobins of the *rue Saint-Honoré*; the second . . . meets in the *Grands-Augustins*. Both have a numerous membership; both are a source of uneasiness for Parisians from the influence they may have over the National Assembly."¹¹⁵ Lindet, in a letter to his brother, expressed it even more simply, "A singular division reigns in the Assembly: the hall has become a battlefield where two enemy armies face one another."¹¹⁶

But who were these two armies? What differences can one find in the character and composition of their respective contingents? Unfortunately, the participants in the Breton Club and the Monarchien group will probably never be known for certain. Membership in the Jacobin group can be generally reconstructed, however, through the research of Alphonse Aulard.¹¹⁷ Based on this source, 205 Constituent Assembly deputies would seem to have adhered to the "Amis de la Constitution" in the months following December 1790. Although membership had undoubtedly varied somewhat since the club first formed in late 1789, this number is surprisingly close to the round figure of 200 who were supporting Jacobin candidates for committee assignments in April 1790.¹¹⁸ As for the "Capuchins Society," a substantial portion of its participation can be ascertained from the petition signed during the faction's meeting on April 19.¹¹⁹ Even though the specific object of the petition, the maintenance of Catholicism as the sole state religion, may have prevented the association of a few anticlerical conservatives—like the Marquis de Ferrières—the petition remains the best single record of factional adhesion to the Capuchins for the first half of 1790. In all, 292 deputies signed this document—suggesting an alignment on the Right significantly larger than the Jacobin group.¹²⁰

A preliminary analysis of the two groups of deputies suggests that the collective biographies of the Capuchins, on the one hand, and the Jacobins, on the other, were in certain respects dramatically different.¹²¹ Without a doubt, the most salient distinction was the remarkable alignment by Old Regime estate.

¹¹⁵ Quoted in Walter, *Histoire des Jacobins*, 93–94.

¹¹⁶ Lindet, *Correspondance*, 38.

¹¹⁷ The most complete list of the Jacobins—as Aulard himself indicates—is to be culled from the index at the end of his *La Société des Jacobins*, vol. 6. This must be used to complement the list of December 1790, published in the introduction of his vol. 1.

¹¹⁸ See above, note 112.

¹¹⁹ *Déclaration d'une partie de l'Assemblée nationale sur le décret rendu le 13 avril concernant la religion* (Paris, 1790).

¹²⁰ The petition was signed by 293, but one deputy later retracted his signature. A part of the group around Malouet and Clermont-Tonnerre seems also to have remained aloof from the Capuchins for tactical reasons; Malouet, *Mémoires*, 2:41–43. Of the core group of Monarchiens listed by Egret who were still active as deputies in April 1790, eleven of seventeen signed the petition.

¹²¹ More detailed analysis must await the completion of Edna Lemay's biographical dictionary.

More than eight out of ten Jacobins were deputies of the Third Estate, while more than nine out of ten Capuchins represented the two privileged orders.¹²² Indeed, among the handful of twenty-three Third Estate deputies belonging to the Capuchins, a third were actually nobles or clergymen who had been elected by the commoners.¹²³ To be sure, the single largest contingent of Capuchins—slightly over half—were clergymen. They included almost all of the bishops, as well as some 40 percent of the parish priests in the Assembly.¹²⁴ We have already noted the importance of opposition to the Constituent Assembly's religious policies in the general cohesion of the group. It is not surprising that the coalition soon came generally to be known as "les noirs." Yet if one includes the deputies from all three estates, some 54 percent of the Capuchins are found to have come from noble families. And of these, two-thirds were true "aristocrats" who could trace back their lineage to the sixteenth century or earlier.¹²⁵ Much can be made of the relatively modest family backgrounds of the three most visible leaders of the Right: Maury, Cazalès, and Eprémèsnil—the first a commoner, the second two of first-generation nobility. Yet one should also not overlook the strong "aristocratic" imprint on the Capuchins as a whole—aristocratic not only in the revolutionary meaning of "conservative ideology" but with the older implication of ancient social or caste origins. Beyond Maury, Cazalès, and Eprémèsnil, seven of the ten most common Capuchin speakers originated in families of this kind.¹²⁶

As for the Jacobins in the Assembly, the commoners among them—the vast majority—differed very little in their social contours from the Third Estate deputies as a whole.¹²⁷ There was, however, a distinct over-representation of those calling themselves "avocats" and a corresponding under-representation of the various categories of royal officeholders.¹²⁸ Unfortunately, the socioeconomic position of the "lawyers" in question is difficult to define and was almost certainly very diverse—from practicing court lawyers to wealthy landowners who

¹²² One hundred sixty-eight (82 percent) of 205 Jacobins. Two hundred sixty-nine (92 percent) of 292 Capuchins. The importance of the deputies' estate in Left/Right alignments has been noted by Applewhite, "Political Alignment," 267–68.

¹²³ Seven nobles and one *chanoine* (canon).

¹²⁴ One hundred sixty-two (55 percent) of the signers were clergymen. This included 102 (49 percent) of all 207 curés sitting at this time, and thirty-four (87 percent) of the thirty-nine bishops.

¹²⁵ One hundred sixty-seven (57 percent) of the 292 are known to have been nobles. One hundred and seven (64 percent) of the 167 could trace their lineage before 1600. Of course, the nonaligned nobles in the Assembly would have had much the same lineage breakdowns. I am not arguing that aristocratic origins determined political options but that they may have been a factor in such options, and that the social origins of the Capuchins were strongly weighted by a large bloc of aristocratic deputies, a bloc that was clearly in evidence to contemporaries.

¹²⁶ Marquis de Folleville, Vicomte de Mirabeau, Comte de Virieu, Marquis de Foucauld-Lardimalie, Chevalier de Murinais, Marquis d'Ambly, and Bishop de Bonal. Among the Capuchin group, by my count, Maury spoke the most, Folleville was second, and Cazalès was third, while Eprémèsnil was eighth.

¹²⁷ For instance, the proportion of all Third Estate deputies with agriculturally related professions was 12 percent; it was the same for Jacobin deputies from the Third. For all deputies from commercial professions: 12 percent; for Jacobin deputies: 14 percent.

¹²⁸ Thirty-two percent of all deputies called themselves "avocats," while 42 percent of Third Estate deputies in the Jacobins described themselves in this way. Thirty-five percent of all deputies were officeholders—including 17 percent who were judges; while 26 percent of the Jacobins were officeholders—including 12 percent who were judges.

had never set foot in a court and whose law degrees were essentially symbols of status.¹²⁹ The paucity of officeholders among the Jacobins is not insignificant, however, in that many of them—particularly the royal magistrates—occupied what was perhaps the highest status level of the entire Third Estate. They were also among those Commoners deputies with the greatest vested interests in the Old Regime. In all, only eleven clergymen and forty-two noblemen—representing any of the three estates—had thrown in their lots with the Jacobins. A few of the nobles—the Duc d'Aiguillon, the Vicomte de Noailles, the brothers Lameth, for example—were from among the greatest families of the kingdom. Of the central club leadership, both Alexandre Lameth and the parliamentary magistrate Adrien Duport had been members of the second estate. It is significant, nevertheless, that this small group of Jacobin nobles was distinctly less “aristocratic” than the large contingent of Capuchin nobles, with only a little over one-third holding titles dating before the seventeenth century.¹³⁰ Indeed, almost half of all the newly ennobled Third Estate deputies—eighteen of thirty-eight—joined the Jacobins. Among the twelve most important leaders of the Constituent Assembly from the Jacobin deputies, those participating most frequently in National Assembly debates, only one—the Comte de Mirabeau—was an “aristocrat” by birth.¹³¹

Beyond the question of social differences, a preliminary prosopography would suggest two other ways in which the two political factions can be distinguished. In the first place, Capuchins and Jacobins would seem to have had somewhat differing residences. A full 26 percent of the Capuchins, compared to only 11 percent of the Jacobins, are known to have lived in Paris. Most of the Parisian Capuchins in question were in fact from great noble families who had won election in provincial *bailliages* by virtue of their names and status. Half of the Jacobins, by contrast—compared to only 30 percent of the Capuchins—came from small to medium-size provincial towns, with populations of from 2,000 to 50,000 inhabitants. Among those Capuchins who did come from the provinces, however, a significantly larger proportion came from southern France—south of a line between La Rochelle and Geneva—and notably from the Massif Central and other interior provinces of the Midi, regions that were among the most isolated and economically backward in the kingdom.¹³²

In the second place, the Capuchins were distinctly older, on the average, than their opponents. Among those deputies for whom dates of birth are known, the Jacobins averaged 43.2 years old in 1790, three years younger than the average

¹²⁹ See Lenard R. Berlanstein, *The Barristers of Toulouse in the Eighteenth Century, 1740–1793* (Baltimore, 1975), 11, 16.

¹³⁰ Sixteen (38 percent) of 42.

¹³¹ The others, in order, were Charles-François Bouche, Le Chapelier, Jean-Denis Lanjuinais, Gaultier de Biauzat, Barnave, Jacques Defermon, Guillaume-François-Charles Goupil de Prefelne, Jean-François Reubell, Duport, Philippe-Antoine Merlin, Pierre-Louis Prieur, and Robespierre.

¹³² By my count, 48 percent of the Capuchins and 30 percent of the Jacobins represented districts south of this line. Approximately 30 percent of all deputies seem to have come from southern France; Edna Lemay, “La composition de l’Assemblée constituante: Les hommes de la continuité?” *Revue d’histoire moderne et contemporaine*, 24 (1977): 349.

for all deputies, while the Capuchins averaged 49.5.¹³³ Indeed, among the totality of the youngest deputies, those under thirty at the beginning of the revolution, no less than 40 percent adhered to the Jacobins—compared to 11 percent who associated with the Capuchins. Among those deputies over sixty-five at the opening of the Estates General, 28 percent became Capuchins and only 5 percent became Jacobins. Moreover, such differences cut across all three orders: the average ages of Jacobin clergymen, Jacobin noblemen, and Jacobin commoners were all lower than their respective counterparts within the rival faction. The differences were particularly dramatic among the small group of radical nobles, whose mean age was nearly ten years younger than that of their noble colleagues on the right side of the hall.¹³⁴ A generational effect was clearly operational in the radicalism and conservatism of many of the deputies.

FACTIONAL CONFRONTATIONS BETWEEN LEFT AND RIGHT continued as a characteristic feature of the Constituent Assembly to the very end of that body's existence in September 1791. Indeed, to judge by the participation index (see the graph), the principal speakers on the Right were never more active than during the spring of 1791.¹³⁵ Yet the political influence of the conservative coalition within the Assembly was ebbing sharply by the middle of 1790. Two extraordinary successes by the Patriots at the beginning of the summer undoubtedly contributed to breaking the momentum and the energy of the Capuchin-Impartial alliance: the formal suppression of the nobility on June 20 and the passage of the Civil Constitution of the Clergy three weeks later. Taken together, the two measures fostered a deep sense of fatalism and demoralization on the part of many of the deputies of the Clergy and the Nobles.¹³⁶ The last president elected by the Right retired from office in the middle of July. From September of that year—after a succession of moderate Patriots—the Jacobins effectively came to dominate the presidency, as they already controlled the secretariat, through the end of the Constituent Assembly. By November, the Jacobin curé Lindet could write to his brother, "The aristocracy no longer has an influence, it seems to me, on the choice of Assembly officers."¹³⁷

Yet the rise of the Jacobins to preeminence within the National Assembly had been neither rapid nor inevitable. Their triumph, if triumph it was, came not in

¹³³ Ages are taken primarily from Robert, *Dictionnaire*.

¹³⁴ Among the Clergy, the Jacobins averaged 45.4 years, the Capuchins averaged 50.7; among the Nobles, the Jacobins were 37.0, the Capuchins 46.5; among the Third Estate, the Jacobins were 42.3, the Capuchins 43.7. Murphy and Higonnet, "Les députés de la noblesse," 240, noted the relative youth of the "liberal" nobles.

¹³⁵ After Louis XVI's attempted flight from the kingdom in June 1791 and the majority's decision to suspend the king's powers, however, much of the Right boycotted the debates.

¹³⁶ There are many comments on the demoralization of the bishops; Bouchette on the archbishop of Aix, *Lettres*, 484; and Boisgelin, speaking for himself in his letter of late June, Boisgelin de Cucé, *pièce* 114, AN, M 788; Ménard de la Graye on the bishop of Le Mans, May 28, 1790, 10 J 122, AD, Sarthe; also Bryce, *Un évêque d'ancien régime*, 269–70, on the bishop of Nancy. Among the Capuchins in general, eight are known to have left the Assembly in the first half of 1790, thirteen in the second half of 1790, and twenty-eight in the first half of 1791.

¹³⁷ Lindet, *Correspondance*, November 22, 1790, 247.

1789—as is usually suggested—but only in the second half of 1790.¹³⁸ And the chronology is significant. Events during that first formative year of the revolution helped set the tone of the parliamentary process and establish many of the basic political presuppositions for years to come. Far from capitulating, the representatives of privilege and conservatism had asserted a dynamic presence within the Estates General and the National Assembly from the very beginning. Many of the reactionary deputies of the Clergy and the Nobles, whose initial intransigence had greatly contributed in crystallizing the revolutionary sentiment of June 1789, had eventually been won over by the successful organizational achievements of the Monarchiens and, rapidly adapting to circumstances, had set out in an alliance with the more moderate conservatives to exploit to their advantage the new system and its rules. Learning from the methods of the Breton Club, this coalition had soon taken the initiative, pioneering many of the electoral tactics usually attributed to the Jacobins, and playing a key role in the transformation of the more archaic Old Regime faction into a first sketch of the modern political party. In their heyday, their numbers closely matched—were actually somewhat superior to—those of the Jacobins, and they could feel justified in their ambition to win over a sufficient number of the nonaligned moderate majority to seize control of the Assembly. The Monarchien Malouet certainly believed this was possible, and he speculated, many years later, on what might have happened if a relatively small number of deputies on the Right had not decided to abandon the battle so early in the revolution. The Jacobin leader Alexandre Lameth made much the same argument, musing that the presence in the Estates General of deputies from the Breton nobility and upper clergy—groups that had boycotted the elections in the spring of 1789—might have entirely transformed the situation.¹³⁹

Inevitably, the Patriots were intensely aware of the offensive of the Right and often, understandably, felt harried and besieged. They were also clearly conscious of the social composition of the group that opposed them at the other end of the hall. It was not a question of mere rhetoric, of the Jacobins concocting imaginary machinations by the aristocrats. In fact they faced genuine, genealogically certified aristocrats, swords at their sides, day after day in the Assembly itself: aristocrats who, for a time, were prominent elements in a highly organized political faction or alliance of factions, and who, for a time, could harbor the plausible hope of a “legal” counterrevolution engineered through the Constituent Assembly itself.¹⁴⁰ Little wonder that the Patriots on the Left soon felt compelled to match the organization of the Right with their own, highly

¹³⁸ It is not possible here to follow the political developments of the Assembly through its completion in September 1791. Significantly, it was only *after* the Right had effectively collapsed as a power within the Assembly that major splits began to appear within the Left coalition—splits that were clearly in evidence before the king’s attempted flight. See, especially, Michon, *Essai sur l’histoire du parti Feuillant*, 182–85.

¹³⁹ Malouet, *Mémoires*, 2:36; Lameth, *Histoire de l’Assemblée constituante*, 1:421. Lindet said much the same thing in a letter of May 8, 1790; *Correspondance*, 155.

¹⁴⁰ Compare the thesis of Sutherland in his *France, 1789–1815*. As I have argued here, I would push back the inception of the revolutionary-counterrevolutionary dialectic to the very beginning of the Estates General and the National Assembly.

centralized party organization. Little wonder that many deputies on the Left came to view all opposition parties as dangerous and illegal, and that the very concept of a "loyal opposition" failed to develop in the early revolution. Obviously, a close "internal" analysis of this kind does not answer all the questions about the dynamics of the revolution, or even about the dynamics of the National Assembly. A more general synthesis will have to take into account those exogenous factors—economic trends, crowd activities, international relations, and the pressures of newspapers, clubs, home constituencies, and Parisian assemblies—all those forces that exerted an overwhelming impact on revolutionary developments as a whole. A broader account will also have to confront the seemingly intractable problem of the influence of prerevolutionary ideologies on the men of 1789.

But the approach taken here does reveal the extent to which the political behavior of a significant and highly influential—if minority—segment of the National Assembly was associated with social divisions among the deputies. To be sure, the social divisions operative were not those of class. Most of the Nobles and most of the wealthy commoners who represented the Third Estate—as revisionist historians never tire of demonstrating—had basically similar relationships to the means of production.¹⁴¹ A Marxian analysis, whatever its utility for explaining other aspects of the revolution, is ultimately not very useful for the problems of the National Assembly. It seems likely that, for understanding social interaction within the Constituent Assembly, an analysis based on a complex of categories—such as wealth, status, education, and previous political experience—will prove far more helpful. In terms of the subjective element of status within the traditional value system—a value system with which the deputies, as revealed in their letters, long maintained an ambiguous relationship—there was clearly a world of difference between the majority of those individuals participating in the two major factional divisions of the Assembly.¹⁴² And it seems evident that the political-social dialectic between Left and Right, a dialectic whose origins can be traced to the earliest days of the Estates General and the National Assembly, would exert a major influence on the development of the new political culture of the French Revolution and of modern France.

¹⁴¹ See the classic study of Alfred Cobban in *Aspects of the French Revolution* (New York, 1968), 100–02, 109–11. See also George V. Taylor, "Noncapitalist Wealth and the Origins of the French Revolution," *AHR*, 72 (January 1967): 469–96; and Lemay, "La composition de l'Assemblée constituante."

¹⁴² We have seen above that Capuchins and Jacobins also represented, in part, different generations. Although it would be impossible to demonstrate here, they probably also differed significantly in education and overall wealth.

Seventeenth-Century Crisis in Brandenburg: The Thirty Years' War, The Destabilization of Serfdom, and the Rise of Absolutism

WILLIAM W. HAGEN

If a peasant, no matter whose, absconds without securing a substitute to farm his master's or Junker's property, the authorities shall, upon request, pursue him without fail, wherever he may be, as was agreed of old in consultation with the Estates; but he who wants the return of the runaway must send people of his own to bring him back. This rule also applies if a farm servant absconds and is subsequently apprehended.

Ordinance of Joachim II, Elector of Brandenburg, November 3, 1550.¹

We, Frederick William, . . . have received many complaints about the pride and insolence of the domestic servants, the peasant farmers and the farm workers . . . , and that they refuse to conform to Our previous ordinances and edicts; instead they do as they please, and through their contrariness, stubbornness and all manner of aggravations make themselves almost intolerable to their masters.

Preamble: Revised Statute on Peasants, Servants, Livestock Herders, and Shepherds, December 18, 1681.²

WHICH OF THESE PRONOUNCEMENTS FROM THE THRONE casts the truer light on the peasant-lord relationship in early modern Brandenburg-Prussia? Historians prefer the first. It conjures up the image of enserfed farmers on degraded tenures and manorial laborers dragooned into service, both looking to escape through flight. It invokes also the alliance of Crown and nobility, jointly and amicably exercising their powers of repression to compel the villagers to toil on the Junkers', that is, the landed gentry's, manor farms. But if lordly coercion

The formulation of this essay's argument benefited from discussions of earlier versions presented in papers read at the 1986 meeting of the American Historical Association and, in 1987, in the colloquium of the Zentralinstitut für Sozialwissenschaftliche Forschung at the Freie Universität Berlin. Grants from the University of California, Davis, the Deutscher Akademischer Austauschdienst, the National Endowment for the Humanities and, above all, the Alexander von Humboldt-Stiftung supported this research project and the related work noted below. My thanks to these forums and institutions and to the helpful staff of the Geheimes Staatsarchiv, West Berlin.

¹ Walter Friedensburg, ed., *Kurmärkische Ständeakten aus der Regierungszeit Kurfürst Joachims II (1535–1571)*, 2 vols. (Munich-Leipzig, 1913–1916), 1: 834.

² Christian Otto Mylius, ed., *Corpus Constitutionum Marchicarum*, 6 vols. (Berlin, 1737–1751 [hereafter, CCM]), 5.3.1, no. 21, col. 141.

shaped the social landscape, so too were the powers of noble lordship and the state itself defined in part by the peasant refractoriness and insubordination condemned in Frederick William's ordinance.

This assertion combines uneasily with the literature both on the crisis of the seventeenth century and the rise of absolutism in Brandenburg-Prussia. Here the peasant appears as a sorely abused victim. But why join together these two historiographical debates? This article aims primarily to reinterpret the social origins and context of emergent absolutism in seventeenth-century Brandenburg, the heartland of the Prussian monarchy of the eighteenth century. The Thirty Years' War plunged Brandenburg into social crisis. The Great Elector Frederick William (reign, 1640–1688), architect of Prussian absolutism, confronted and created conditions of political crisis, both at home and abroad. Although historians of Brandenburg-Prussia have not entered the debate on the "general crisis" of the seventeenth century, the social interpretation of absolutist politics conveyed in their work has figured importantly in it and continues to influence strongly large-scale discussions of the seventeenth century. The new balance sheet this article presents of the social costs and benefits of early absolutism in Brandenburg thus carries implications beyond the German frontiers into a broader realm of European history.

Some historians derive the seventeenth-century crisis from the exhaustion of the growth trend of the long sixteenth century. In their view, demographic expansion accompanied by declining agricultural productivity and deteriorating real wages in town and country alike loom up as principal causes of this conjunctural reversal. As the price of bread rose, demand for manufactures slackened. Capital flowed into seigneurialism, which sought to profit from rising ground rents or to squeeze a land-hungry peasantry. In some analyses, the nobility multiplied and stratified, bringing forth a faction interested in the acquisition of peasant rents and subjects through foreign or civil war. In any case, the pathological end-phase of sixteenth-century economic growth created social and political instabilities issuing in the wars, demographic reverses, and ensuing economic stagnation or depression that, in the material realm, constitute the seventeenth-century crisis. In the sphere of politics, the crisis manifested itself in conflicts that, in many European lands, found their steely resolution under a regime of absolutism. For those among the nobility allied to the Crown, this outcome spelled rescue and the rewards of office and patronage. For the peasantry, it entailed degradation through taxation and, in Europe east of the Elbe, tightened bonds of personal serfdom and redoubled seigneurial exploitation.³

³ In this vein, see E. J. Hobsbawm, "The Crisis of the Seventeenth Century," *Past and Present*, 5–6 (1954), rpt. in Trevor Aston, ed., *Crisis in Europe, 1560–1660* (London, 1965), 5–58; Ruggiero Romano, "Between the Sixteenth and Seventeenth Centuries: The Economic Crisis of 1619–1622," (1962) in Geoffrey Parker and Lesley M. Smith, eds., *The General Crisis of the Seventeenth Century* (London, 1978), 165–225; Miroslav Hroch and Josef Petrán, *Das 17. Jahrhundert—Krise der Feudalgesellschaft?* (Hamburg, 1981 [Czech original: 1976]), a work offering a good survey of the literature on the crisis; Heiner Haan, "Prosperität und Dreissigjähriger Krieg," *Geschichte und Gesellschaft*, 7 (1981): 91–118; Wilhelm Abel, *Agrarkrisen und Agrarkonjunktur: Eine Geschichte der Land- und Ernährungswirtschaft Mitteleuropas seit dem hohen Mittelalter*, 3d edn. (Hamburg, 1978), 122 and following; Peter

To this interpretation, emphasizing (in Niels Steensgaard's terms) a crisis of production, other historians have opposed an approach stressing a crisis in distribution.⁴ Whether or not the European economy was beginning to choke on its sixteenth-century contradictions, the seventeenth-century crisis derived independently from the social and political pressures generated by Renaissance and Baroque state-building. Military and courtly profligacy drained capital from the private sector, ruining numerous bourgeois (but also noble) financiers. It led, especially in the seventeenth century, to the taxation of the peasantry to—and sometimes beyond—the threshold of starvation. The rise of the Leviathan state not only provoked rebellions, variously successful and hopeless, but also entailed the demographic and economic reversals, evident by the mid-seventeenth century, that in some lands did not yield to a new cycle of growth for another century. The birth throes of the modern state gave rise to a crisis from which in many countries it emerged in absolutist vigor, dominating an exhausted society. The state administration proceeded, in alliance with its aristocratic and bourgeois partisans, to turn the fiscal screws on the common people, especially the peasantry.

Both these interpretive tendencies have enlisted in their support the authoritative works on early absolutism in Brandenburg-Prussia. In the West German and Anglo-American literature, the forceful arguments of F. L. Carsten and Hans Rosenberg still occupy commanding positions. In the German Democratic Republic, a Marxist analysis has taken shape that tends to complement rather than contradict them. Common to both approaches is the assumption that the power and interests of the Junker nobility set the limits to the evolution of state and society in early modern Brandenburg-Prussia. Both hold that the foundation of the absolutist regime rested on a compromise struck in 1653 between the Elector Frederick William and the Brandenburg nobility. In the *Landtags-Recess* or parliamentary agreement of that year, the Junkers granted Frederick William the taxes necessary to field a standing army, the institution whose development brought in its train the bureaucratized autocracy of the eighteenth-century Prussian state. Frederick William rewarded the nobility for their acquiescence in this political revolution by strengthening their domination of the peasantry, whom the agreement of 1653 bound more securely in the shackles of serfdom than before the Thirty Years' War. What the nobility gave up in the way of

Kriedte, *Peasants, Landlords and Merchant Capitalists: Europe and the World Economy, 1500–1800* (Cambridge, 1983 [German original: 1980]), chap. 2; on the seventeenth-century crisis in the literature of neoclassical and Marxist economic history, see William W. Hagen, "Capitalism and the Countryside in Early Modern Europe: Interpretations, Models, Debates," *Agricultural History*, 62 (Winter 1988): 13–47.

⁴ Niels Steensgaard, "The Seventeenth-Century Crisis," (1970), in Parker and Smith, *General Crisis*, 42 and 26–56, *passim*. See also H. R. Trevor-Roper, "The General Crisis of the Seventeenth Century" (1959), in Aston, *Crisis in Europe*, 59–95; Perry Anderson, *Lineages of the Absolutist State* (London, 1974), 15–59, 195–235, 397–431; Theodore K. Rabb, *The Struggle for Stability in Early Modern Europe* (Oxford, 1975), chap. 9 and *passim*; Charles Tilly, "Reflections on the History of European State-Making" and "Western State-Making and Theories of Political Transformation," in Charles Tilly, ed., *The Formation of National States in Western Europe* (Princeton, N.J., 1975), chaps. 1 and 9; J. H. Elliott, "Yet Another Crisis?" in Peter Clark, ed., *The European Crisis of the 1590s* (London, 1985), 301–11.

aristocratic self-government and co-sovereignty was balanced by an increase in their seigniorial powers in their rural bailiwicks.⁵

In Rosenberg's words, Frederick William "confirmed and enlarged" the Junkers' "customary fiscal, economic, and social privileges and [their] *de facto* freedom to tyrannize the tillers of the soil and the rural craftsmen . . . In consequence, the basic social institution of agrarian Prussia, peasant serfdom, increased in severity until the latter part of the eighteenth century." "Abject poverty" and "helpless apathy" were the fate of the common people. In Carsten's formulation, Frederick William used "the Junkers' class interests to win them over to an alliance with the crown . . . The peasant-serfs were too down-trodden to revolt, and anyhow they were more oppressed by their [Junker] masters than by the government."⁶

In the German Democratic Republic, Günter Vogler and Klaus Vetter hold that the transition from the "veiled noble dictatorship" of the pre-absolutist regime (*Ständestaat*) to "the open dictatorship of one representative of the nobility in the interest of the entire noble class" served the "objective" end of maintaining the functional capability of the late-feudal state in the face of the rise of European capitalism and the sharpening class conflicts accompanying it. Brandenburg-Prussia required an absolutist armature to survive within the European state-system dominated by the western powers. Domestically, Junker manorialism provoked, if not large-scale peasant revolts, then at least the "lower forms" of peasant class struggle such as the shoddy performance or even refusal of *corvée* labor on the Junker estates. The settlement of 1653 guaranteed that the power of the absolutist regime in which the Junkers acquiesced would serve to

⁵ This argument, underpinning what might be called the compromise theory of Brandenburg-Prussian absolutism, was advanced with characteristic vigor by Otto Hintze in "Die Hohenzollern und der Adel" (1913), rpt. in Otto Hintze, *Regierung und Verwaltung* (Göttingen, 1967), 39 and 30–55, *passim*. See also Hintze's *Die Hohenzollern und Ihr Werk*, 7th edn. (Berlin, 1916), 205. The compromise theory lies also at the heart of F. L. Carsten's *Origins of Prussia* (Oxford, 1954), especially part 3, and Hans Rosenberg's *Bureaucracy, Aristocracy, and Autocracy: The Prussian Experience, 1660–1815* (Boston, 1958), esp. chap. 1. See also Hans Rosenberg, "Die Ausprägung der Junkerherrschaft in Brandenburg-Preussen, 1410–1618," in his *Machteliten und Wirtschaftskonjunkturen* (Göttingen, 1978), 24–82, which offers a revised version of "The Rise of the Junkers in Brandenburg-Prussia, 1410–1653," *AHR*, 49 (1943):1–22, 228–42. The compromise theory is emphatically advanced in Hans-Ulrich Wehler, *Deutsche Gesellschaftsgeschichte, I: 1700–1815* (Munich, 1987), 229, 589 n. 43; compare 142–43.

⁶ Rosenberg, *Bureaucracy*, 45, 48. Carsten, *Origins*, 275, 277. Carsten's and Rosenberg's interpretations of the political dynamics of Prussian absolutism are cast in doubt by the work of the West German historians Peter Baumgart, Klaus Deppermann, Peter-Michael Hahn, and Gerd Heinrich. Yet all of these historians accept that the absolutist regime rested on a compromise securing or strengthening the Junkers' domination of the peasantry. See Peter Baumgart, "Wie absolut war der preussische Absolutismus?" in Manfred Schlenke, ed., *Preussen: Beiträge zu einer politischen Kultur* (Berlin, 1981), 89–105; Klaus Deppermann, "Der preussische Absolutismus und der Adel: Eine Auseinandersetzung mit der marxistischen Absolutismustheorie," *Geschichte und Gesellschaft*, 8 (1982): 550–53; Peter-Michael Hahn, *Struktur und Funktion des brandenburgischen Adels im 16. Jahrhundert* (Berlin, 1979), 168 and *passim*; Peter-Michael Hahn, "Landesstaat und Ständetum im Kurfürstentum Brandenburg während des 16. und 17. Jahrhunderts," in Peter Baumgart, ed., *Ständetum und Staatsbildung in Brandenburg-Preussen* (Berlin, 1983), 53 and 41–79, *passim*; Peter-Michael Hahn, *Fürstliche Territorialhoheit und lokale Adelsgewalt: Die herrschaftliche Durchdringung des ländlichen Raumes zwischen Elbe und Aller (1300–1700)* (Berlin, 1986), esp. 239–57; Gerd Heinrich, *Der Adel in Brandenburg-Preussen* (Darmstadt, 1965), 295 and 259–314, *passim*.

enforce, until Napoleon's time, a harsher and more profitable form of peasant serfdom.⁷

The compromise theory of early absolutism in Brandenburg reigns unchallenged from east to west in part because research has so far confined itself mostly to the political history and sociology of the central government and the corporate nobility. While there are some good local studies of noble manors and peasant villages, the social and economic history of early modern Brandenburg-Prussia has only begun to be written in terms acceptable to present-day scholarship. The economic dynamics of the peasant-landlord relationship, especially during the seventeenth century, lack precise formulation. The alliance of Crown and nobility against the peasantry has been deduced from a one-sided reading of the political evidence, while its efficacy in practice has yet to be put to a satisfactory empirical test. Instead, the literature pays an unearned tribute to the Junkers by exaggerating their coercive powers over the peasantry and assuming that they exerted them effectively in practice. These suppositions are essential to the prevailing interpretations, Marxist and non-Marxist alike, of Prussian absolutism as a political revolution from above that advanced, or at least upheld, the economic interests of the landed nobility. But these suppositions are mistaken.

IN 1626, FOREIGN ARMIES OF OCCUPATION brought the afflictions of the Thirty Years' War to the Mark Brandenburg. Did they descend on a society gripped by structural crisis? The historical literature has addressed this problem only obliquely, by weighing the consequences of the spectacular rise in the sixteenth century of the Junker-dominated system of large estates worked by a subject peasantry and by assessing the social costs of the Brandenburg Electors' fiscal embarrassments and depredations.⁸

⁷ Günter Vogler and Klaus Vetter, *Preussen: Von den Anfängen bis zur Reichsgründung* (Berlin, 1979), quotation from 41; see also 31–33, 44. Similarly, Anderson, *Lineages*, part 2, chaps. 1–3. Compare Gerhard Heitz, "Der Zusammenhang zwischen den Bauernbewegungen und der Entwicklung des Absolutismus in Mitteleuropa," *Zeitschrift für Geschichtswissenschaft*, 13 (1965): 71–83.

⁸ On the agrarian economy before the Thirty Years' War, see (both on the substantive issues and the historical literature) Hartmut Harnisch, "Die Gutsherrschaft in Brandenburg: Ergebnisse und Probleme," *Jahrbuch für Wirtschaftsgeschichte*, 4 (1969): 117–47; Hahn, *Struktur*, part 1; and William W. Hagen, "How Mighty the Junkers? Peasant Rents and Seigniorial Profits in Sixteenth-Century Brandenburg," *Past and Present*, 108 (1985): 80–116. On princely finances and the urban economy, see Siegfried Isaacsohn, "Die landständischen Verhältnisse in den Marken bis zum Jahre 1640," in his edn. of *Urkunden und Aktenstücke zur Geschichte des Kurfürsten Friedrich Wilhelm von Brandenburg: Zehnter Band: Ständische Verhandlungen: Zweiter Band: Mark Brandenburg* (Berlin, 1880), 10.2: 1–29; Fritz Kaphahn, *Die wirtschaftlichen Folgen des 30jährigen Krieges für die Altmark* (Gotha, 1911); Martin Hass, *Die kurmärkischen Stände im letzten Drittel des sechzehnten Jahrhunderts* (Munich-Leipzig, 1913), esp. 135–284; Hugo Rachel, Johannes Papritz, Paul Wallich, *Berliner Grosskaufleute und Kapitalisten*, 2 vols. (1934–38; rpt. edn., Berlin, 1967), vol. 1; Helmuth Croon, *Die kurmärkischen Landstände 1571–1616* (Berlin, 1938); Hahn, *Territorialhoheit*, 188–201. On pre-war trends in the larger German economy, Friedrich Lütge, "Die wirtschaftliche Lage Deutschlands vor Ausbruch des Dreissigjährigen Krieges" (1958), in Hans Ulrich Rudolf, ed., *Der Dreissigjährige Krieg: Perspektiven und Strukturen* (Darmstadt, 1977), 458–547; Theodore K. Rabb, "The Effects of the Thirty Years' War on the German Economy," *Journal of Modern History*, 26 (1962): 40–51; Hermann Kellenbenz, *Deutsche Wirtschaftsgeschichte*, 2 vols. (Munich, 1977–81), 1: 212–95; Friedrich-Wilhelm Henning, *Das vorindustrielle Deutschland 800 bis 1800* (Paderborn, 1974), 179–232.

The century before 1618 undoubtedly belonged to the Junkers, or rather to those among the nobility with demesne land and peasant subjects in sufficient supply to make possible the profitable sale via the Baltic or Hamburg of grain and livestock products on capacious western markets. In the period 1560–1620, grain exports from Brandenburg were proportionally as great as Poland's. The Junkers enriched by such commerce frequently lent their earnings to the impecunious Electors in exchange for offices and on the security of pawned incomes from the extensive Crown lands. In this way, an oligarchy arose that, at least until the beginning of the seventeenth century, successfully mediated the interests of the Crown and the landed nobility and endowed the political system with a stability uncommon for the age.⁹

The secular trend of rising grain and livestock prices at home and abroad, so favorable to the Junkers, continued up to and beyond the outbreak of the war, despite a downward dip at the beginning of the seventeenth century. The yearly incomes or leases of noble properties rose accordingly. The selling prices of noble estates soared, sometimes outstripping the capitalized value of anticipated annual returns. Not surprisingly, the turnover of Junker properties accelerated in the half-century before the war, a trend that a princely edict of 1573 condemned but could not halt.¹⁰

The rising frequency of estate sales forced on Junker families to satisfy their own creditors, or those of their friends and relatives for whom they had stood surety, testified to the perils of unbridled consumption and the proliferation of dealings on credit, of which the Electors set an example unwisely followed by the moneyed classes. To make matters worse, Brandenburg, like most other German and European lands, passed in the years 1618–1622 through a wrenchingly inflationary monetary devaluation (the *Kipper- und Wipperzeit*), in which many creditors were badly clipped or ruined. These developments, representing the transfer or destruction of accumulated profits, bore witness to the limited opportunities for capital investment and the inadequacy of state revenues characteristic of the age. But they are not in themselves signs that the Junkers' estate agriculture had lost its profitability, especially since one nobleman's loss was often another's gain.¹¹

⁹ On agricultural exports, see Harnisch, "Gutsherrschaft," 121–24. On the nobility's social composition and credit dealings, see Hahn, *Struktur*. Hahn calculated that the Junker oligarchy comprised no more than the upper third of the noble lineages. The more typical nobleman had to content himself with a manorial farm and peasant *corvées* and rents securing him a rustic gentility with limited access to the export market. Beneath this level stood the noble poor (27–28, 48–49, 64, 206 and following, 239); Croon, *Die kurmärkischen Landstände*, 147.

¹⁰ On agricultural commodity price trends, see Abel, *Agrarkrisen*, appendix, table 2, and p. 188. The rising annual income or lease value (reckoned in a stable money of account) of the Neugattersleben estates in the Magdeburg region illustrates the trend on the market for large landed properties: 1573—3,000 Taler; 1596—5,500 Taler; 1617—6,300 Taler. Hahn, *Struktur*, 66; compare 344–45; on the accelerating pre-war turnover of Junker estates, 37–49, and Kaphahn, *Altmark*, 25. Estate values were optimistically appraised in the late sixteenth and early seventeenth centuries by capitalizing annual net income at 3–4 percent. See Geheimes Staatsarchiv, West Berlin (hereafter, GStA.), Provinz Brandenburg, Rep. 2A: Kurmärkische Kriegs- und Domänenkammer: Domänenregistratur: Amt Eldenburg, Paket 1, no. 20 (1573: 3 percent); GStA. Provinz Brandenburg, Rep. 37: Gutsarchiv Stavenow (hereafter, GStA. Stavenow), no. 255 (1601–14: 4 percent).

¹¹ Hahn (*Struktur*, 48–49; "Landesstaat," 63; *Territorialhoheit*, 24, 155, 196) interprets the numerous early seventeenth-century noble bankruptcies and forced estate sales to mean that the private

So long as the commodity price trend remained favorable, the question facing the Junkers was whether or not they could maintain (or increase) their production for the market. They had, in the course of the sixteenth century, brought under cultivation virtually all their demesne land as well as the “deserted” village land (*wüste Feldmarken*) they had appropriated during the late medieval agrarian crisis. They had equipped themselves with the human and animal muscle necessary to harvest their fields by imposing weekly labor services of, typically, two or three days on their peasant subjects—the proper farmers or full peasants (*Vollbauern*) with largeholdings and teams of horses and oxen performing their *corvées* by plowing and hauling, the cottagers with small holdings (*Kossäten*) supplying manual labor. To secure as cheaply as possible the full-time manorial servants necessary, in addition to the *corvée*-bound peasantry, to the operation of their estates, the Junkers had prevailed on the government to rule that the sons and daughters of the largeholding peasantry, if not required as workers on the parental farm, could be compelled to serve at the manor one or more years for room and board and low statutory wages (*Gesindezwangsdienst*).¹²

In this way, the Junkers’ appropriation of the peasant surplus had, on the eve of the Thirty Years’ War, reached a level adequate to ensure the operation of their estates in their existing form, though not without considerable production costs in the form of manorial teams, servants’ wages, and, in many cases, food and drink given the peasants on their days of service at the manor. Any attempt to raise peasant rents, and especially to increase the odious labor services, risked strikes or communal appeals to princely adjudication. Overriding the Junkers’ objections, the higher courts regularly heard such cases in the pre-war decades. Their inclination to confine peasant rents to levels regarded as locally customary was undoubtedly a brake on the arbitrariness of individual Junkers, even if the landlords’ collective pressure on their peasant subjects tended to determine the burden of rent in the various regions of the land.¹³

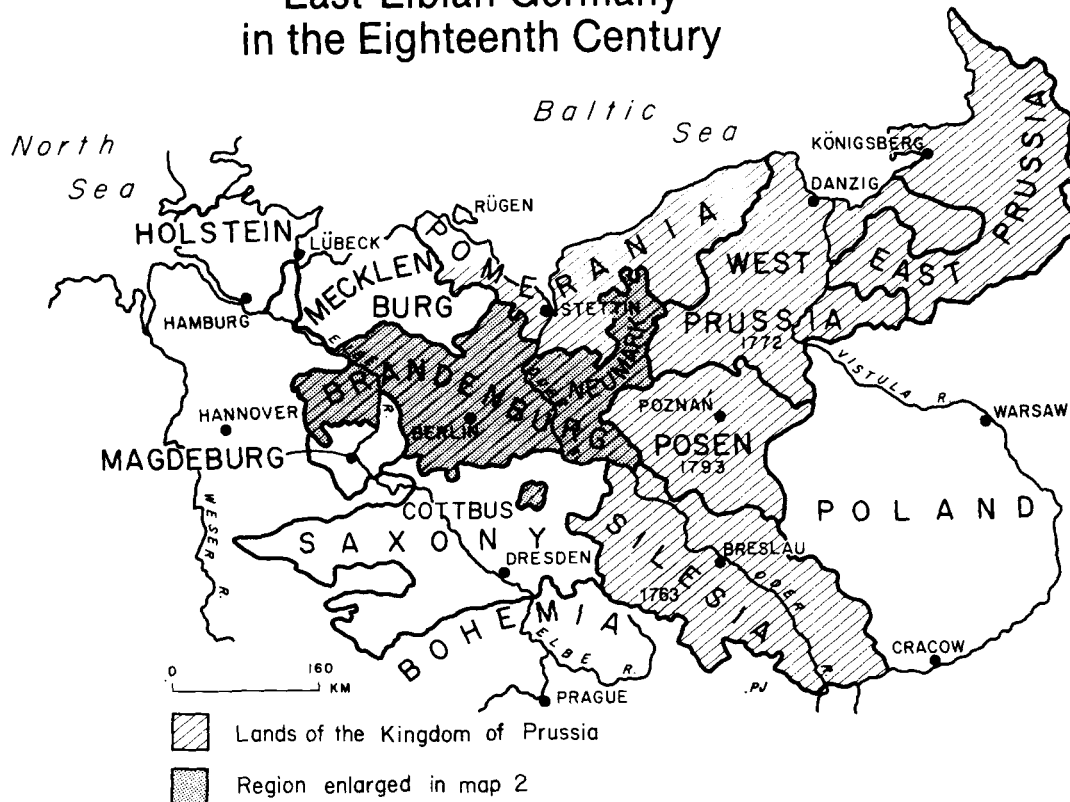
landlords as a class had fallen into economic crisis at the end of the sixteenth century. But the ready purchase at high prices of overindebted estates by capital-strong and credit-worthy noblemen would seem to argue for property redistribution within the nobility rather than generalized pre-war crisis. In 1620, debts forced the family von Rohr to sell their Freyenstein estates. The von Winterfeld family bought them for the considerable sum of 153,000 Taler, an acquisition they retained into the nineteenth century. The frequency of such nonspeculative transactions remains unknown. Gerhard Albrecht, *Die Gutsherrschaft Freyenstein* (Dissertation. Pädagogische Hochschule Potsdam, Historisch-Philologische Fakultät, 1968), 31. On the *Kipper- und Wipperzeit* in Brandenburg, see Rachel Grosskaufteute, 1: 379–91.

¹² On labor services, see Hagen, “How Mighty the Junkers?” On the legal conditions of compulsory manorial service, see Friedrich Grossmann, *Über die gutsherrlich-bäuerlichen Rechtsverhältnisse in der Mark Brandenburg vom 16. bis 18. Jahrhundert* (Leipzig, 1890); and Ernst Lennhoff, *Das ländliche Gesindewesen in der Kurmark Brandenburg vom 16. bis 19. Jahrhundert* (Breslau, 1906). On material conditions, see William W. Hagen, “Working for the Junker: The Standard of Living of Manorial Laborers in Brandenburg, 1584–1810,” *Journal of Modern History*, 58 (1986): 143–58. On the size of *Vollbauern* farms, see note 20, below.

¹³ Acting collectively as village communes, the Brandenburg peasantry, except in those few regions of strict serfdom (on which, see text below), frequently brought suits in the pre-war decades against their landlords, especially in questions of rents and labor services. See Grossmann, *Über die gutsherrlich-bäuerlichen Verhältnisse*, chap. 3 and *passim*; Helga Schultz, “Bäuerliche Klassenkämpfe zwischen frühbürgerlicher Revolution und Dreissigjährigem Krieg,” *Zeitschrift für Geschichtswissen-*

MAP 1

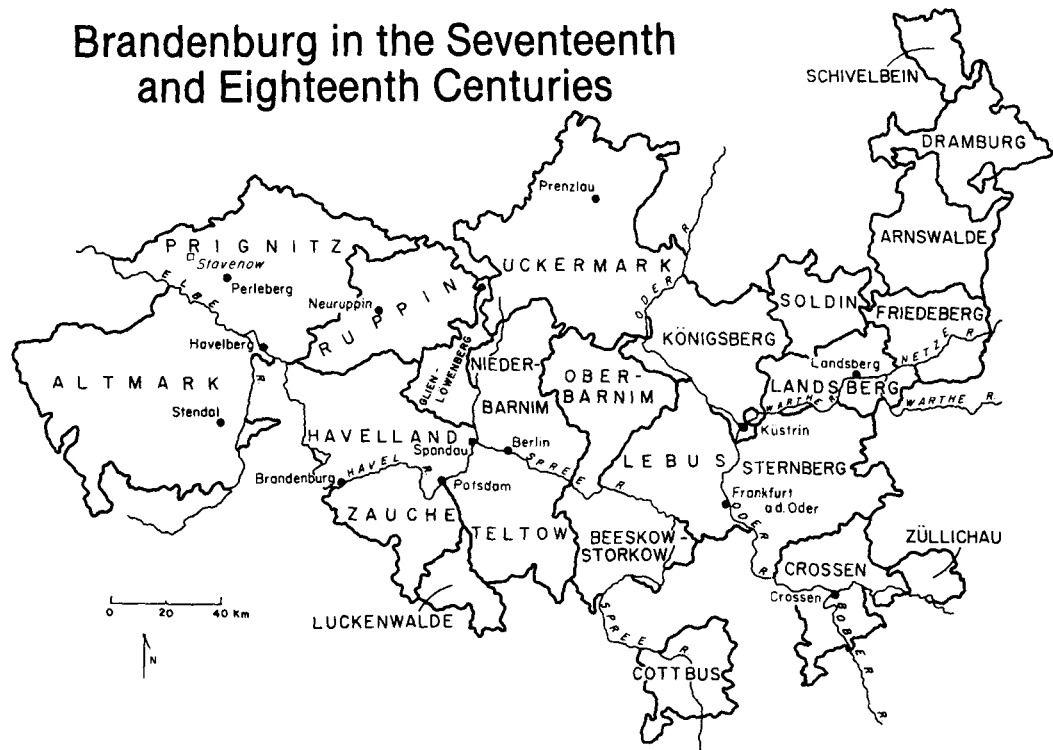
East-Elbian Germany in the Eighteenth Century



The richer Junkers, having ratcheted up their subject peasants' rents as high as seemed necessary or possible, turned toward buying up the properties of their improvident or unlucky colleagues and investing their manorial earnings in income-bearing princely offices. In the short run, neither of these tactics threatened the stability of the pre-war agrarian structure. But the poorer nobility could not play by these rules. Their interest lay in acquiring estate land or broadening their domanial acreage, and the most tempting way to do so was by the acquisition of peasant holdings. In the Uckermark and a few regions of the Neumark (see Maps 1 and 2), a strict personal serfdom (*Leibeigenschaft*) prevailed, binding the peasantry through heredity to the soil and rendering their farm occupancy insecure and purely usufructuary. But, in most parts of Brandenburg, the peasantry were personally free and held their farms firmly in hereditary tenure. Their Junkers could not summarily evict them and enclose their farms and acreage into the seignorial demesne. However, in 1540, the nobility pressured the government into confirming the Junker's right to buy out,

schaft, 2 (1972): 156–73; Hartmut Harnisch, "Klassenkämpfe der Bauern in der Mark Brandenburg zwischen frühbürgerlicher Revolution und Dreissigjährigem Krieg," *Jahrbuch für Regionalgeschichte*, 5 (1975): 142–72.

MAP 2



at the locally current price, peasant holdings under his jurisdiction in the event that he required land on which to erect a manor house for his own habitation. This concession opened the door to the quasi-legal expropriation of peasant land (*Bauernlegen*).¹⁴

In the sphere of peasant-landlord relations, the question of peasant expropriation, alongside the Junkers' efforts to restrict the peasantry's access to the appellate courts, generated the sharpest political controversy in the half-century before the war. While, to the lesser nobility, the enclosure into their demesnes of one or two peasant farms meant economic invigoration, to the princely regime and the higher nobility co-responsible for its financing, it signified a shrinkage in the number of peasant holdings, the principal units of taxation in the countryside. This shrinkage threatened to burden the remaining peasantry with higher taxes and so work against the Junkers' interest in skimming the peasant surplus themselves. Moreover, since the noble estates depended crucially on labor-rents, the number of peasant holdings could not be willfully reduced without forcing up the number of *corvées* levied on the surviving subject farmers. If these

¹⁴ Grossmann, *Über die gutsherrlich-bäuerlichen Verhältnisse*, 15. The government also conceded the Junkers' right to evict obdurate and disobedient peasants from their holdings but not to enclose such vacated farms into their tax-free demesnes.

farmers balked, the Junkers would face a self-induced labor shortage requiring them to raise their investments in manorial horsepower and wage labor.

For these reasons, the corporate nobility and the princely regime attempted in the late sixteenth and early seventeenth centuries to limit the passage of peasant land into Junker hands.¹⁵ Carsten nevertheless held that peasant expropriations represented a major trend in the pre-war decades. Yet the prime statistical source, the Mittelmark cadastre of 1624, shows only that some 7.3 percent of the land of the largeholding peasantry (or *Vollbauern*) had been enclosed into noble demesnes in the previous half-century. The frequency of enclosures was indeed rising, but, in 1624, 82 percent of taxable village land still remained in peasant hands.¹⁶

The villages were not suffocating under the weight of feudal rent and princely taxation. Local studies indicate that the post-medieval rise in seignorial rent ended with the nobility's imposition of the more-or-less oppressive sixteenth-century labor services. Heavy though this tribute often was, it did not sever the landed peasantry from production for the market, whose price trends favored them as well as their Junkers. Peasant farms on productive soils paid profits, as evidenced by interest-earning deposits of occasionally considerable sums in the coffers of the urban tax-corporations (*Städtekassen*). But such modest successes were not typical, and a better gauge of the landed peasantry's economic health would be the extent of their livestock holdings. Local and regional studies suggest that these were at least adequate to the maintenance and reproduction of the peasant household and, in many cases, strong enough to ensure profitable sales.¹⁷

¹⁵ Grossmann, *Über die gutsherrlich-bäuerlichen Verhältnisse*, 25; Kaphahn, *Die wirtschaftlichen Folgen*, 22; Croon, *Die kurmärkischen Landstände*, 85.

¹⁶ F. L. Carsten, "The Origins of the Junkers," *English Historical Review*, 243 (1947): 164–65, 178. The 1624 cadastre, on which the calculations in the text above are based, was published by Grossmann, *Über die gutsherrlich-bäuerlichen Verhältnisse*, 102–38; see also 28, 71. It notes the dates of peasant expropriations in two-thirds of the 242 villages where they took place. Among 170 known cases, 32 percent occurred before and 68 percent after the year 1600. Nevertheless, Grossmann's figures on the Mittelmark (71) show that, between 1570 and 1624, the number (not the area) of full peasant holdings (*Bauernstellen*) declined by only 5 percent, that of landed cottagers (*Kossäten*) by 2 percent. The figures on the distribution of peasant and estate land cited in Hagen, "How Mighty the Junkers?" 108, should be understood as applying to village lands only. Hans Goldschmidt's crude estimates of the distribution in 1618 of all land (that is, village and demesne lands combined) placed 60 percent between the Elbe and Oder in the possession of the peasantry and the towns, 40 percent in that of the nobility and Crown. In the Neumark, his ratio was 54:46. Data cited in Johannes Schultze, *Die Mark Brandenburg*, 5 vols. (Berlin, 1961–69), 5: 173. In 1710, among all of Brandenburg's 1,967 villages, 64 percent stood under noble jurisdiction, 33 percent under the Crown, and 3 percent under urban magistrates. Kurt Breysig, *Geschichte der brandenburgischen Finanzen in der Zeit von 1640 bis 1697: Darstellung und Akten: Erster Band* (Leipzig, 1895), 192.

¹⁷ On the peasantry's engagement in market production, see Walter Naudé, *Die Getreidehandelspolitik und Kriegsmagazinverwaltung Brandenburg-Preussens bis 1740* [= Acta Borussica, *Getreidehandelspolitik*, vol. 2] (Berlin, 1901), 32 and following; Hugo Rachel, *Die Handels-, Zoll- und Akzisepolitik Brandenburg-Preussens bis 1713* [= Acta Borussica, *Handels-, Zoll- und Akzisepolitik*, vol. 1] (Berlin, 1911), 71, 86–87; Hass, *Die kurmärkischen Stände*, 162–63; Harnisch, "Gutsherrschaft," 125–26, and, on peasant savings, 137 and following. On strong livestock holdings among the peasantry, see Hartmut Harnisch, *Die Herrschaft Boitzenburg* (Weimar, 1968), 94; GStA. Provinz Brandenburg, Rep. 16, no. 16; Werner von Kiekebusch, *Geschichte des Klosters Heiligengrabe seit der Reformation* (unpub. ms., 1949), 296; GStA. Provinz Brandenburg, Rep. 37: Gutsherrschaft Kletzke, no. 1, pp. 84 and following (*Amtsbuch* of 1649, reflecting pre-war conditions). See also Grossmann, *Über die gutsherrlich-bäuerlichen Verhältnisse*, 48–49; and Hahn, *Territorialhoheit*, 346–47. On peasant well-being in Mecklenburg, Friedrich Mager, *Geschichte des Bauerntums und der Bodenkultur im Lande Mecklenburg*

While the impartibility of peasant holdings worked against immiseration through morcellation, it also condemned many peasant children to landlessness. Yet the Mittelmark cadastre of 1624 does not project a picture of actual or impending overpopulation: for every single or married rent-paying lodger (*Hausmann*) earning a living by wage labor, there were more than sixteen landed peasants, the majority with large holdings. For every lodger, there were three more-or-less economically secure households of village artisans, fishermen, and livestock handlers.¹⁸

If it is difficult to discern a pre-war trend toward generalized impoverishment and overpopulation in the Brandenburg countryside, there is clear evidence of the sporadic harvest failures and epidemics, unhappy characteristics of the age, which could temporarily destroy any rustic well-being that might have accumulated. The whole land was closed to grain exports in 1571–1572 and 1603–1604, sure signs that subsistence crisis had overridden the Junkers' lust for gain. There were widespread harvest failures in 1597 and 1623. Plague repeatedly struck not only the towns but also the countryside. Yet these scourges were intermittent and displayed no clear tendency toward worsening.¹⁹

It is, finally, hard to argue that taxation was stripping the villages of their substance. The Estates were periodically obliged to accept responsibility for retiring the Electors' debts, and the nobility for its part accordingly levied a direct tax (of variable weight) on its peasant subjects. But the principal agencies of the nobility responsible for collecting and administering these funds succeeded until the outbreak of the war in raising their quotas without incurring the rising indebtedness into which borrowing drove the urban corporations. If the villagers had not paid their public charges, the nobility would either have had to tax themselves, which they sometimes did, or carry a permanent funded debt, which, despite occasional borrowing, their chief agencies in the Mittelmark and Altmark-Prignitz districts never accumulated. As late as 1614, the nobility could afford to advance the government 210,000 Taler; yet, in 1623, both their

(Berlin, 1955), 120–22; in Magdeburg, Hartmut Harnisch, *Bauern—Feudaladel—Städtebürgertum* (Weimar, 1980), chap. 9.

¹⁸ Calculations based on Grossmann's data, *Über die gutsherrlich-bäuerlichen Verhältnisse*, 138. The cadastre did not count the itinerant and disorderly poor, whose numbers may have been considerable. In their appraisal of the Stavenow estates in 1601, the brothers von Quitzow wrote of the lodgers living in peasant outbuildings (*Die Einwohner der Spiker Kerll*): "of no value; some way should be found to rid the villages of them"; GStA. Stavenow, no. 255. On beggars and vagrants, Schultze, *Mark Brandenburg*, 4: 145; Kaphahn, *Die wirtschaftlichen Folgen*, 29–30. Yet, in 1608, the corporate nobility of the Altmark district complained they could not hire permanent farm workers or occasional laborers without offering them six meals daily during the harvest season and four the rest of the year. Schultze, *Mark Brandenburg*, 4: 172–73. Compare Friedensburg, *Kurmärkische Ständeakten*, 1: 839–43; CCM, 5.3.1, no. 5, cols. 14–15. Whatever the trend in the growth of the rural population may have been, the corporate nobility and the princely administration did not find the landlords oversupplied with steady and compliant farm laborers.

¹⁹ On harvest failures, Naudé, *Die Getreidehandelspolitik*, 29 and following, 76; Croon, *Die kurmärkischen Landstände*, 183. On plague and other epidemics, Kaphahn, *Die wirtschaftlichen Folgen*, 38–39, 73; Johannes Schultze, *Die Prignitz* (Köln-Graz, 1956), 173 and following. Kaphahn (73) detected a falling birth rate in the pre-war Altmark villages. He also (28) found many Altmark peasants deeply in debt to local burghers. The extent of peasant indebtedness throughout Brandenburg is unknown. The wage statute of 1620 self-righteously blamed the problem of runaway peasants on irresponsibly contracted debts: CCM, 5.3.1., no. 5, col. 15.

principal treasuries were in the black. Pre-absolutist Brandenburg was, from the Electors' point of view, a tax-starved state, and it is unlikely that its fiscal impositions on the villages were ruinous.²⁰

The long sixteenth century did not smile on the towns of Brandenburg. The Junkers dealt them hard blows by setting up rival breweries in the countryside and bypassing their wholesalers in favor of merchants abroad, especially in Hamburg. Moreover, the nobility forced the politically vulnerable towns to shoulder an increasingly heavy share of the princely debt. Nevertheless, the troubles of the urban tax-corporations should not be taken as evidence of an all-encompassing fiscal crisis of the state, especially considering the good condition of the nobility's tax-collecting agencies. The towns' interest-bearing obligations on the eve of the war must have approximated 2.5 million Taler, an imposing sum of capital that had been accumulated in the private sector, not only by the middle and upper nobility but also by the bourgeoisie and prosperous villagers.²¹

Before concluding that the evidence discussed so far does not lock together in a pattern of economic crisis, it remains to consider the trends displayed by another set of valuable, if also fragmentary, pre-war data: the returns on customs duties. Since 1569, the Brandenburg Junkers had been obliged to consent to a princely levy on their grain exports. This New Grain Duty (*Neues Kornzoll*) paid the Crown, in the years 1584–1589, a yearly average of 19,250 Taler; but the same figure from nine years in the period 1609–1624 amounted to only 12,500 Taler. While the customs rates actually levied in practice and the degree of evasion of payment are uncertain, these data suggest a faltering of grain production for export, an ill omen for the Junkers. Further, the duties on manufactures collected in the 1609–1624 period exceeded the grain levy by 50 percent. The supposition seems justifiable, in view of the modest capabilities of

²⁰ The yearly expenditures of the Electors' household and court in the early seventeenth century amounted to some 150,000 Taler; Hahn, *Struktur*, 306 n. 866. The two most important tax corporations (*Hufenschosskassen*) were those of the nobility of the Mittelmark and of the Altmark-Prignitz districts. The analogous Kasse in the Uckermark accumulated a deficit. On the finances of these institutions, see the works cited in note 6, above. At the *Hufen- und Giebelschoss* tax-rates of 1624, a typical largeholding peasant with two hides (*Hufen*) of arable land (= roughly 75–85 acres) would have paid an annual direct tax of 2.25 Taler, which, at average prices in Berlin in the period 1624–52, amounted to the value of three bushels of rye. Grossmann, *Über die gutsherrlich-bäuerlichen Verhältnisse*, 102; prices averaged from data in Naudé, *Die Getreidehandelspolitik*, 568–69. On peasant farm productivity, see Table 4, below.

²¹ On the urban economy, see Rachel, *Grosskaufleute*, vol. 1; Carsten, *Origins*, 136–48; Kaphahn, *Die wirtschaftlichen Folgen*, 6–9; Friedensburg, *Kurmärkische Ständeakten*, 2: 305–12. The financial accounts of the urban tax corporations tell a tale of overall economic stagnation rather than headlong decline. In the period 1571–1620, the principal indirect tax on urban consumption (the *Neues Biergeld*) yielded annually from 40,000 to 60,000 Taler, with no discernible falling tendency. In the same period, the three principal urban *Kassen* together paid out a yearly average of 141,000 Taler in capital and interest: data from five years between 1605 and 1625, if taken as representative of the entire period, yield annual payments of 140,000 Taler. But, while the global sums of these payments remained unchanged, the rate of funded-debt retirement was falling, and the annual interest payments were rising. The stagnating tax base of the *Kassen* forced them to service rather than liquidate the loans they had contracted with the public. Yet, although the devaluation of the early 1620s forced them temporarily to stop payments, they were still in business in 1625. Calculations from data in Hass, *Die kurmärkischen Stände*, 352–60; and Hahn, *Territorialhoheit*, 185, 200. See also Croon, *Die kurmärkischen Landstände*, 198; and Kaphahn, *Die wirtschaftlichen Folgen*, 12–21.

the Brandenburg towns, that most of these manufactures were imports rather than exports. Probably they were, in large measure, luxury goods destined for the households of the nobility. If so, the pre-war balance of trade was moving strongly toward a deficit. Among the Junkers, consumption must have been outrunning production. The deficit could have been financed by the export of accumulated Junker profits, but it is likely as well that many noblemen were buying luxury goods on credit. In either case, the trend, if unreversed, must have ended in a throttling of consumption at best and flurry of bankruptcies of indebted Junkers and their local merchant suppliers at worst.²²

In sum, in the pre-war decades, many of the Junkers were living beyond their means. If their exportable surplus was not in fact shrinking, neither was it likely that it could be significantly increased. Their credit calculations were based on the expectation that prices on export markets would remain favorable, whereas the international price trend would, in a few decades, suffer a secular reverse. Pairing these developments among the nobility with the fiscal difficulties and lack of commercial-industrial dynamism in the urban sector yields a picture of a lopsidedly agrarian economy approaching the end of a long growth cycle. But, before 1618, or even 1626, it would be an exaggeration to say that Brandenburg was gripped by a structural crisis. Its reserves of liquid and fixed capital were considerable, although the security of deposits in the tax corporations depended on revenues soon to be preempted by foreign armies. Above all, the Junkers' estates and the peasant villages were intact and functional. Commercialized agriculture had paid the nobility excellent returns in the preceding several generations. They felt no menace from a rebellious peasantry. When in 1620 foreign troops first passed through the land, arms were issued to the farmers of the Altmark and turned in without incident when the emergency had passed.²³ Nor did the deepening of the military crisis persuade the Junkers that the constitution of the state should be altered so as to raise up a prince bristling in arms, a role for which in any case Elector Georg Wilhelm (reign, 1619–1640) had no ambition.

THE JUNKERS' AND THE PRINCELY REGIME'S PACIFISM cost them and their subjects dearly. In 1640, two years before the end of the war's violence and plunder, the population in the provincial towns had fallen from pre-war levels by 80 percent, in Berlin-Cölln by 40 percent, and in the countryside from 90 percent in the worst-ravaged districts to 20–40 percent elsewhere. As late as 1652, half the farms of Brandenburg west of the Oder river were deserted. In 1660, half the houses in the towns of the Neumark, east of the Oder, stood empty. Before the war, the far-flung state domains had drawn rents and *corvées* from over 6,000 large and small holders and had paid over 160,000 Taler annually into the treasury. After the war, the number of state peasants had declined by nearly half, domain revenues by more

²² Rachel, *Handels-, Zoll- und Akzisepolitik*, 20, 32; calculations from data, 842.

²³ Isaacsohn, *Urkunden und Aktenstücke*, 10.2: 329. Compare Schultze, *Mark Brandenburg*, 4: 205, 212–13, 221.

than two-thirds. The losses of available workers and income on the private estates of the Junkers must have been similarly extreme.²⁴

Such are the gloomy statistics in which historians have sought to take the toll of the Thirty Years' War. But, in their search for measures of destruction, their eyes have passed over one of the most significant causes and consequences of the great upheaval's material damage. In the war-torn countryside, the villagers looked in vain to their Junkers for protection against fighting and plunder, and the hunger and disease following in their train. Seigneurial authority collapsed over wide regions, if not everywhere. The peasantry, thrown back on their own devices, ceased to obey their noble overlords. The repressive machinery of the system of serfdom, whose efficiency before 1618 was far from perfect, froze up during the war.

It is strange that this turn of events should have escaped historians' attention, for the government acknowledged it repeatedly. The statute on laborers and wages issued in 1635 for the Altmark forbade large and small holders to settle anywhere without the permission of the local seigneur (whether Junker, princely official, or town corporation): "this abuse . . . has spread so far that the peasantry presume, just as they wish, to move from one village to another." Instead of staying on their holdings and rendering their seigneurial dues and services, "as far as is still possible," they were leasing or sharecropping other people's land and paying nothing to their rightful overlords.

Villagers and townspeople alike had, "in these very hard times of war, betaken themselves, secretly or openly, to other places." Many had armed themselves illegally. Numerous farm workers refused to enter into yearly contracts and dealt instead in horses and livestock plundered by marauding soldiers, a trade the edict of 1635 tried to abolish. Those who remained in the Junkers' pay negotiated agreements (*Pacta*) with their employers, illegally raising wages above the statutory maximums by such subterfuges as claiming shares of the manorial harvests, requiring their masters to raise livestock for them, and extorting gifts in cash from them at the holidays or even for their weekend carouses (*Wochen-Zechen*). Rural artisans and day laborers were unilaterally raising their own wages and making excessive demands for food and drink, which the edict attempted to limit to three meals daily.²⁵

While forbidding the workers' excesses, the statute of 1635 paid silent tribute to the soaring cost of labor by prescribing a new schedule, superseding that of 1551, of pay in cash and kind for day laborers and for manorial servants employed throughout the year. Such wage statutes reveal little about workers' real earnings. Even if their employers insisted on observing the prescribed limits

²⁴ The demographic consequences of the war are controversial. Population figures for 1640 from Schultze, *Mark Brandenburg*, 4: 287–88; for 1652 and 1660 from Günther Franz, *Der Dreissigjährige Krieg und das deutsche Volk*, 4th edn. (Stuttgart, 1979), 22–23. On the state domains: Grossmann, *Über die gutsherrlich-bäuerlichen Verhältnisse*, 68; Breysig, *Geschichte der brandenburgischen Finanzen*, 238–39. Compare Wehler, *Deutsche Gesellschaftsgeschichte*, 1: 54.

²⁵ CCM, 5.3.1. no. 9, cols. 23–36, *passim*; quotations from cols. 27–30, 33–35. The transgressions and disobedience of the rural population, denounced in this and the other edicts on which the present essay draws, were doubtless not everywhere prevalent. But the government would hardly have singled them out for legislative remedy if they had not been widespread.

on payments in cash, the value of income supplements in kind—housing, gardens and arable land, grazing rights, fuel, food and drink, clothing—was harder to fix in law and could decisively affect wage levels. Nevertheless, the statutory rates of pay in cash are a barometer of the Junkers' reactions to conditions in the labor market, and it is not surprising that they found themselves obliged in 1635 to recommend considerable increases in the legal maximums. Thus, in the Altmark, the average annual money wages payable to the highest category of agricultural worker on the Junker estates—the *grosser Ackerknecht*, who was responsible for plowing, team-driving, and mechanical repairs—were raised in 1635 (in terms of a stable money of account) 99 percent above their 1551 level.²⁶

Analogous statutes promulgated for the Mittelmark and Neumark in the years 1644–1646 show that wartime depopulation had driven up the cost of labor across the board. The offenders of 1635 are joined by millers, the higher sort of manorial employees (such as vintners, fishermen, and field foremen), threshers, linen-weavers, shipmasters, raftsmen, and transporters on land of persons and goods. The wages in cash and, in some cases, in kind of these and many other rural workers were fixed anew. In two districts of the Mittelmark where pre-war rates are known, the average annual money wages of *grosse Ackerknechte* now stood 82 percent higher than in 1551. The edict of 1645 acknowledged that, in the heavily depopulated Uckermark and Neumark, where strict serfdom was in whole or part the law, the Junkers were offering illegally high wages, injurious to their brethren elsewhere, in their efforts to bring their estates back into production.²⁷

The increasingly heavy direct taxes levied since 1626 on the landed peasantry impeded the resettlement of abandoned holdings. Yet both the princely regime and the Junkers had a vital interest in repopulating the villages, especially with largeholding *Vollbauern* maintaining plow and haulage teams for *corvée* labor on the Crown and private estates. There were many farmers who had stood their ground during the war or who, having fled, were willing to return to their patrimonies. Others, from inside the country as well as from neighboring principalities, were interested in taking a Brandenburg farm. Under these conditions, a tendency toward compromise emerged in the war's aftermath: the government tried to adjust its tax levies to levels that were locally bearable, while

²⁶ Pay scales for 1635, CCM, 5.3.1, cols. 32–34; for 1551, Friedensburg, *Kurmärkische Ständeakten*, 1: 839. The government promulgated these and the other wage statutes discussed in the present essay according to the wishes of the corporate nobility expressed at the local or district level. For brevity's sake, the fluctuations in the pay in cash of *grosse Ackerknechte* are taken here as representative of the movement of the money wages of all rural laborers. The cash earnings of less robust and highly skilled men and of most women were considerably lower.

²⁷ CCM, 5.3.1, no. 11 (1644), cols. 37–56; no. 12 (1645), cols. 55–76; no. 13 (1646), cols. 75–110; compare CCM, 5.3.1, no. 10 (1641), cols. 35–38. To the abuses condemned in 1635, the new edicts added the breakaway of the shepherds, and hence of the profitable wool trade, from seigneurial domination. See Otto Meinardus, ed., *Protokolle und Relationen des Brandenburgischen Geheimen Rates aus der Zeit des Kurfürsten Friedrich Wilhelm*, 6 vols. (Leipzig, 1889–1917), 3: 144–47, 150–54. Wage rates in 1644 (Teltow and Havelland districts): CCM, 5.3.1, no. 11; pre-war rates: Friedensburg, *Kurmärkische Ständeakten*, 1: 816 and following, 827 and following; wage rates in 1645 (Neumark): CCM, 5.3.1, no. 13.

the administrators of the state domains and the Junkers who had rescued some of their substance from the wreckage offered new settlers of peasant farms in their jurisdictions help in the form of free building materials and livestock and reduced claims on rents and labor services. In this way, a devastated large estate such as Stavenow in the Prignitz district could repopulate by 1649 nearly two-thirds of its *Vollbauern* and cottager holdings (see Table 2, below). The downward pressure on peasant rents is evident in the Privy Council's report of 1652 to Frederick William on the misdeeds of a state domain lessee, including his demand that the peasants under his jurisdiction pay from their holdings the full pre-war dues in cash and kind (*Pächte*), "which are nowhere else given or claimed."²⁸

Set against the government's and less hard-pressed nobility's pragmatic inclination to make economic concessions for the sake of repopulating their villages was the impulse, strong among the lesser gentry but felt in some measure by the entire seignorial class, to restore or even broaden their pre-war rights over the peasantry by judicial-administrative compulsion. In the Uckermark, the nobility had looked on helplessly as, during the war, their peasants and manorial servants shook off the bonds of personal serfdom and fled their villages. In response to the Junkers' appeals, Frederick William issued in 1643 an edict restoring in full the old regime in this district. But, in 1644 and again in 1648, the Uckermark nobility complained that their "absconded subjects refused all compliance and would not return to their cabins (*Hütern*)."²⁹

In the Prignitz district, the peasantry suffered the lesser legal disabilities prevailing in most parts of the electorate. Severe wartime devastation had left only 373 peasant and cottager holdings occupied in 1640, but in 1652 there were nearly 1,500 more, restoring the landed peasantry to 40 percent of its pre-war numbers.³⁰ This resettlement occurred in a tense atmosphere of peasant-landlord conflict. In 1643, the corporate nobility protested the Prignitz peasantry's filing of what amounted to a class-action suit before the high court (*Kammergericht*) in Berlin against excessive rents, labor services, and taxes. The Privy Council referred the nobility's petition to the high court, but Frederick William's position was unequivocal: "We cannot in any way approve the peasants' independent undertaking, especially since they do not constitute a corporate body (*Universität*). It looks more like a sedition that, if not stopped in time, might burst out in action." Spurned by the courts, the peasants armed and organized themselves in 1646 against the soldiers still moving through the land, provoking

²⁸ Quotation from Meinardus, *Protokolle und Relationen*, 4: 582–84. For examples of postwar tax remissions granted on petitions of peasants and also state domain administrators, see 2: 74, 296–97, 465, 601, 614; 4:115. On the immigration of foreign peasant settlers, Breysig, *Geschichte der brandenburgischen Finanzen*, 247 and following; Franz, *Der Dreissigjährige Krieg*, 95 and following; and the military census of 1652 in Johannes Schultze, ed., *Die Prignitz und ihre Bevölkerung nach dem dreissigjährigen Kriege* (Perleberg, 1928).

²⁹ Quotation from Meinardus, *Protokolle und Relationen*, 4: 60; see also 2: 440; Schultze, *Mark Brandenburg*, 4: 300 and 5: 36–40. As late as 1687, nearly two-thirds of the Uckermark's pre-war peasant and cottager holdings stood abandoned (although local devastation during the war of 1674–79 partly accounts for this circumstance). Calculation based on Breysig, 240; and Otto Behre, *Geschichte der Statistik in Brandenburg-Preussen* (Berlin, 1905), 179.

³⁰ Schultze, *Die Prignitz*, 208–09.

fear among the nobility that the villagers, who were already refusing to render their dues and labor services, might now stage an armed revolt against them. In 1648, the peasant bands had still not been disarmed. In 1656, an edict appeared announcing the arrest of the ringleaders who had led the Prignitz peasantry's resistance not only to seigneurial rents but also to auxiliary military services and taxes. In one village, sentences to the local jail were still being meted out to the recalcitrant in 1658.³¹

In 1651, the Brandenburg nobility demanded new legislation to bring the peasantry to heel. In the Privy Council's words, the Junkers were complaining "that the statute on laborers and wages promulgated here some years ago absolutely cannot be brought into effect and that the manorial servants, especially the plowmen and drivers (*Knechte*), have to be paid much more than the statute allows." The council confessed to Frederick William that it did not see how this state of affairs, "given the present shortage of hands," could be remedied, "unless some emphatic means of coercing the insubordinate into obedience are agreed upon and strictly applied"—this was the essential point—"in all parts of the land."³²

IN 1652, FREDERICK WILLIAM SUMMONED THE ESTATES of the Altmark and Mittelmark to win them over to his fiscal reforms. The nobility countered with a litany of grievances. They complained that during the war the landlords "did not hold the [peasant farmers] too strictly to the performance of their labor services and the payment of their rents and dues, so that they might the better bear the burden of the heavy taxes and other public charges. But now, some of them, denying they owe any dues at all, are forcing lawsuits [before the appellate courts] over this issue on their landlords (*Gutsherren*)." While the nobility conceded that no one should be deprived of the right of legal redress, they urged the government to support the rulings of seigneurial justice and to punish, with prison or otherwise, peasants and their attorneys in cases of unjustifiable appeals. The Junkers also complained that the government, in repopulating the villages attached to the state domains, was granting new peasant settlers "far too many" years of exemption from taxes and rents. This policy put the private landlords under heavy pressure to follow suit or lose the competition for new subject farmers.³³

³¹ Quotation from Meinardus, *Protokolle und Relationen*, 2: 233; see also 3: 491, 673; 4: 182–83; 5: 144–46. Schultze, *Die Prignitz*, 201–04; CCM, 6.1, no. 126 (1656); GStA. Provinz Brandenburg, Rep. 16, no. 97; Max Wichmann, *Die Höfe von Breddin (Prignitz) von der ersten Erwähnung bis zur Gegenwart* (unpub. ms., 1941), 80.

³² Quotation from Meinardus, *Protokolle und Relationen*, 4: 356–57. At the year's end, the government announced it was preparing a new statute on laborers and that, pending its publication, manorial servants in compulsory service whose terms were now ending could be required against their will to serve another year under their present master; CCM, 5.3.1, no. 15 (1651). Another edict accused the "insolent farm servants" of wishing to flee "the fatherland" for foreign parts. To forestall this, the seigneurial authorities were now empowered to refuse their subjects permission, except for "sufficient reason," to settle under another jurisdiction. On disregard of this ruling, see note 46, below. CCM, 5.3.1, no. 14 (1651).

³³ Grievances and quotation, Isaacsohn, *Urkunden und Aktenstücke*, 10.2: 238.

Against this background, the paragraphs of the *Landtags-Recess* of 1653 defining in law the postwar peasant-lord relationship hardly add up to the abandonment of the villagers to noble abuse so widely deplored in the historical literature. The government promised to reject appeals entered “frivolously” by the peasantry against their lords and to punish groundless complaints with prison terms. But it explicitly upheld the access of all subjects without exception to the Crown tribunals, promised that all appeals would be heard “in case of denial of justice” at the seigneurial level, and refused to intimidate lawyers representing the Junkers’ subjects.³⁴

The nobility requested confirmation of their right to evict “obdurate and disobedient” farmers when circumstances warranted it. But the government ruled that evictions were permissible only “ob grave et enorme delictum,” and then only after a proper legal inquest and assenting judgment of a higher court. The nobility also asked the government to support their efforts, in those districts where strict personal serfdom was customary, to recover those of their subjects “who had stealthily absconded so as to set themselves illegally in the liberty of new lords who had accepted them as their subjects.” The *Landtags-Recess* did indeed uphold the oppressive custom of the Uckermark Junkers. Emancipation there from personal serfdom could not be claimed on grounds of the passage of time but only through “bona fides, titulus, vel scientia et patientia Domini.” This ruling permitted the reimposition of personal serfdom on those who had escaped it during the war, provided their former masters could track them down. Finally, while the government endorsed the nobility’s claims to the full measure of pre-war peasant rents, it canceled all arrears owed by the Crown’s own peasant subjects and urged private landlords to follow its example. Moreover, it put its own interests first in ruling that the collection of unpaid taxes took legal precedence over the claims of private individuals against peasant farms.³⁵

Shortly after the negotiation of the *Landtags-Recess* with the Estates of the Altmark and Mittelmark, the government sealed a *Landes-Recess* with the nobility and towns of the Neumark. There the pre-war condition of the non-servile

³⁴ *Landtags-Recess de dato den 26. Jul. 1653*, CCM, 6.1, no. 118 (hereafter, *Landtags-Recess* 1653), col. 435.

³⁵ Isaacsohn, *Urkunden und Aktenstücke*, 10.2: 273 (quotation), 239, 250; *Landtags-Recess* 1653, col. 438 (quotation), cols. 437, 440, 443–45, 450, 461. The historical literature sometimes holds or suggests, without offering empirical proofs, that the Junkers misapplied the rulings on strict serfdom in the *Landtags-Recess* to deprive freemen or freedmen of their personal liberty. See F. L. Carsten, “Gutsherrschaft und Adelsmacht,” in Schlenke, *Preussen*, 36; Franz, *Der Dreissigjährige Krieg*, 115, 123; Hobsbawm, “Crisis,” 25, 35–37; Henry Kamen, “The Economic and Social Consequences of the Thirty Years’ War,” *Past and Present*, 39 (1968): 57; Gerhard Schormann, *Der Dreissigjährige Krieg* (Göttingen, 1985), 124–25; Wehler, *Deutsche Gesellschaftsgeschichte*, 1: 74, 162. But, while the question requires further study, it is unlikely that enservment occurred on any large scale. In 1652, the Privy Council cited scripture against any such abuse. Isaacsohn, 10.2: 272–73. Both before and after 1653, the government heard appeals against false attributions of servility. See, for example, Meinardus, *Protokolle und Relationen*, 5: 626. In an edict for the Neumark of 1653 (CCM, 6.1, no. 120), the government declared that “where strict serfdom (*Leibeigenschaft*) is not customary it shall not be introduced.” But the government certainly did support the restoration of strict serfdom where it had existed before the war, including numerous villages of state peasants in the Uckermark and Neumark. See CCM, 6.1, no. 204 (1655). In 1644, Frederick William ordered that a stable boy sentenced to the lash (*fustigatio*) be enserved instead. Meinardus, 2: 438.

peasantry had been harder than that west of the Oder. Now it was ruled that, in those cases in which a farmer had two able sons, not only must one succeed his father on the farm, as was customary, but the other could be compelled to take an abandoned holding under his Junker's jurisdiction. Unoccupied farms could also be forced on the numerous able-bodied lodgers living, "in these times of low prices," an undeserved life of ease. The government also soured the peasantry's lives further by sanctioning the nobility's proposals to replace the "onerous feeding" (*beschwerliche Speisung*) of their subjects during manorial service with more or less unpalatable substitutes.³⁶

Whether such measures were well designed to overcome the acute postwar labor shortage in the Neumark may be doubted. During the fighting, a large-scale flight of the peasantry into Poland had occurred, probably especially from the districts of strict serfdom and degraded tenures. It was said in the Privy Council in 1643 that such fugitives "were laughing at those who had stayed behind on their holdings." The *Landes-Recess* of 1653 shows that no progress had been made in negotiating their recovery with the government in Warsaw.³⁷

THE ECONOMIC FORTUNES OF SEVENTEENTH-CENTURY ESTATE AGRICULTURE are represented in Tables 1 and 2, the first assembling the available aggregate data on the Mittelmark and Altmark, the second illustrating the conjunctural trends by the example of the Stavenow estates in the Prignitz district. Both convey a grim picture of wartime losses and a postwar depression so severe as to prevent the restoration of pre-war levels of profitability before the early decades of the eighteenth century. These data suggest that, for three generations after the Thirty Years' War, the most vital interest of the noble landlords and state domain administrators was the reassemblage of their war-decimated labor forces, both of subject peasants and manorial farm servants.

It is not surprising that Tables 1 and 2 both reveal a fairly close correlation between the movement of rye prices, levels of grain production, and net earnings of private and princely estates. More interesting is the perhaps closer relationship between profits and peasant subjects reflected not only in the parallelism of the two series but also, obliquely, in the soaring rates at which the

³⁶ *Churfürstl: Brandenburg: Neumärckischer Landes-Recess, de anno 1653, den 19. August, CCM, 6.1, no. 119 (hereafter, Landes-Recess), cols. 465–78, passim (quotations from cols. 472, 476). In the Neumark, the supernumerary children of the peasantry had been liable since 1572 to unlimited (rather than one to three-year) terms of compulsory manorial service so long as they remained unmarried and without their own households. But there were also numerous peasant holdings in the Neumark that owed the Junkers no more than two days of weekly *corvée*. Compare Christoph Freiherr Senfft von Pilsach, "Bäuerliche Wirtschaftsverhältnisse in einem neumärkischen Dorf (Land Sternberg)," *Forschungen zur brandenburgischen und preussischen Geschichte*, 22 (1909): 127–91.*

³⁷ Quotation from Meinardus, *Protokolle und Relationen*, 1: 621. *Landes-Recess*, cols. 467, 472. In 1654, the government commissioned a Neumark sheriff to investigate illegal labor practices in his district; *CCM*, 5.3.1, no. 16, cols. 133–36. Since "one knows only too well that both master (*Herr*) and servant (*Knecht*) gladly conceal the truth" (col. 133), he was ordered to have all employers and workers swear under oath that they were not paying or receiving excessively high wages in cash or kind. One wonders whether the sheriff had the energy and, when it came to interrogating Junkers in violation of the wage statutes, the nerve for this assignment.

TABLE 1
Economic Trends in the Mark Brandenburg, 1645–1727 (1600 / 1624 = 100)

	1641–50	1651–60	1661–70	1671–80	1681–90	1691–1700	1701–10	1711–20	1721–30
1. Nominal rye price at Berlin	86	96	69	71	67	123	92	116	94
2. Population (Mittelmark and Altmark)	~50				63			105	120
3. Net income from state domains	36	51	23 (=1659)	15	50	59			
4. Revenues from direct taxes (<i>Kontribution</i>)	~70	270	142	182	209	209	164		
5. Appraisal value of dairy cattle	83					267		267	
6. Appraisal value of sheep	150					125		200	
7. Statutory wage of <i>Grossknecht</i> (Mittelmark)	139				190				204
8. Appraisal value of full peasant's labor services	200					300		500	

SOURCES:

- 1 (prices). Pre-1653 prices, Naudé, *Die Getreidehandelspolitik*, 568–69; post-1653 prices, Behre, *Geschichte der Statistik*, 277. The indexes in the table above represent decennial averages: 100 = 1624–1633.
- 2 (population). Behre, *Geschichte der Statistik*, 67, 172, 198 (estimates for the years 1648, 1688, 1713, 1725).
- 3 (state domains). Breysig, *Geschichte der brandenburgischen Finanzen*, 239–240, 285, 372, 376–77 (data for the years 1647, 1651, 1659, 1674, 1681, and 1696). The indexes in the table represent cumulative averages of Breysig's figures, reduced (for 1695/96) by 11 percent to discount administrative costs.
- 4 (revenues). Behre, *Geschichte der Statistik*, 78; Friedrich Wolters, *Geschichte der brandenburgischen Finanzen in der Zeit von 1640–1697*, vol. 2, *Die Zentralverwaltung des Heeres und der Steuern* (Munich, 1915), 575; Rachel, *Handels-, Zoll- und Akzisepolitik*, 857. Data from the years 1650; 1657; 1662 and 1667 (whose indexes of 136 and 147, respectively, are averaged in the table); 1672 and 1677 (whose indexes of 148 and 216, respectively, are averaged in the table); 1682; 1697; and 1710.
- 5–6 (livestock values). GStA. Stavenow, nos. 255 (1614), 57 (appraisal of Prignitz estate of von Capellen family, 1646), 282 (1694), 240 (1717).
- 7 (wages). Friedensburg, *Kurmärkische Ständeaften*, 1:827–38 (wage rates set in 1550); *CCM*, 5.3.1, nos. 11, 12, 21, 36 (wage rates set in 1645, 1681, and 1722).
- 8 (labor services). GStA. Stavenow, nos. 255 (1614), 57 (von Capellen estate, 1646), 282 (1694), and 240 (1717).

TABLE 2
Production and Profits of the Stavenow Estates, 1614–1717 (Appraisal and
Inventory Values, 1614 = 100)

	1647	1649	1666	1675	1694	1717
1. Number of largeholding peasants (<i>Vollbauern</i>) and landed cottagers	29	62		51	78	95
2. Yearly net profit	52		44		79	107
3. Total grain output				33	62	67
4. Sheep and cattle as percent of net profit					233	350
5. Peasant rents and labor services as percent of net profit					254	392

SOURCES: GStA. Stavenow, nos. 255 (1614), 32 (1647), 131 and 43 (1649), 67 (1675), 282 (1694), 240 (1717).

Yearly net profit reckoned for 1614, 1647, and 1717 at 5 percent of estates' market value, for 1666 and 1694 at annual lease price.

landlords assessed (for purposes of property sales) the value of the labor services of settled and working peasant subjects. On the state domains, as at Stavenow, seigneurial incomes followed the melancholy decline of the population during the war. A tendency toward what might have been a rather rapid postwar recovery halted before the resumption of war in the years 1655–1660, during which villagers and townspeople alike suffered the sharp bite of Frederick William's newly increased direct taxation or *Kontribution*. The plundering and destruction of these years must have reversed the population recovery throughout Brandenburg more or less as drastically as it did the settled peasantry at Stavenow.³⁸

In the 1660s and 1670s, estate incomes and grain prices fell to the lowest levels of the seventeenth century. Heavy taxes and localized devastation returned with the war of 1674–1679, the last to torment the country before 1740. Only in its wake did a sustained, if slow, recovery of the population and the profits of landlordism start. The cadastre of the Prignitz villages drawn up in 1686 found 73 percent of all landed holdings in the possession of peasant farmers, while, in the district of Ruppín, the figure was 87 percent. Yet, as late as 1713, the total population of the Mark Brandenburg only just exceeded its 1618 level. The government of Frederick William I (reign, 1713–1740) required long years to wring acceptable yields from the state domains, and, in 1717, the Stavenow estates changed hands for a sum that, in real terms, was some 20 percent lower than what they had fetched in 1614.³⁹

³⁸ For descriptive evidence of the material losses and depopulation occasioned by the war of 1655–1660, see Isaacsohn, *Urkunden und Aktenstücke*, 10.2: 344–45, 489–91; Meinardus, *Protokolle und Relationen*, 5: 469.

³⁹ Prignitz occupancy rate calculated from a sample of fifty villages among a total of 267. Data from Werner Vogel, ed., *Prignitz-Kataster 1686–1697* (Köln-Wien), 1985. On the Ruppín district, Franz, *Der Dreissigjährige Krieg*, 20–21. It cannot be said with certainty when the population of the entire Mark Brandenburg first recovered its pre-1618 numbers. The pre-war population of the

The shortage of working subject peasants meant that the Junkers had to farm their domanial acres, in whole or in part, with hired labor and costly teams of their own. The Junkers and princely estate administrators were driven to this expedient in the second half of the seventeenth century to a degree unrecognized in the literature. At Stavenow in 1694, seigneurial holdings of thirty-six to forty oxen and eight to twelve horses had to be maintained to till but 62 percent of the arable land that had been harvested in 1614 with the help of only twelve manorial oxen. At the end of the seventeenth century, the cost of a manorial labor force of sixteen steadily employed workers was, as a proportion of net seigneurial income, as high as the cost of twenty-seven workers had been at the end of the sixteenth century. Such relatively capital-intensive yet minimally profitable seigneurially financed demesne farms (*Eigenwirtschaften*) were a characteristic feature of the agrarian landscape long after the Thirty Years' War.⁴⁰

A dilemma faced the Junkers at Stavenow and elsewhere. Population contraction and the ruralization of much of the country's urban economic life had weakened domestic demand for cereals at the same time as west German and West European grain export markets were shrinking. The terms of trade in livestock production were more favorable, as Tables 1 and 2 illustrate. From the 1640s, Brandenburg landlords were keen on strengthening their holdings of sheep and cattle. But this entailed considerable investments, beyond the means of many a Junker. Moreover, grain production could be cut back but not abandoned entirely. If local and export prices were low, the postwar landlords' immediate response was still to try to market as much as possible.⁴¹

Neumark is unknown but is unlikely to have exceeded the figure of 115,000 that Behre supplies for 1713; *Geschichte der Statistik*, 198. On state domain revenues after 1713, see Behre, 91–98. GStA. Stavenow, nos. 30, 255 (estate sales).

⁴⁰ GStA. Stavenow, no. 704, fols. 134–36; no. 705, fols. 127–29; no. 43, fol. 40; no. 282, *passim*. The following local studies document the existence of seigneurial *Eigenwirtschaften* during the second half of the seventeenth century and, in some cases, the early decades of the eighteenth century: Harnisch, *Die Herrschaft Boitzenburg* (Uckermark), 138–41; Günter Vogler, "Die Entwicklung der feudalen Arbeitsrente in Brandenburg vom 15. bis 18. Jahrhundert: Eine Analyse für das kurmärkische Domänenamt Badingen" (Uckermark), *Jahrbuch für Wirtschaftsgeschichte* (1966), 1: 156; Albrecht, *Die Gutsherrschaft Freyenstein* (Prignitz), 84–94; Siegfried Passow, *Ein märkischer Rittersitz* (Ober-Barnim), 2 vols. (Eberswald, 1907), 1: 98 and following; Hahn, *Fürstliche Territorialhoheit* (Altmark), 208 and following. To judge from the frequency in the Crown villages of the commutation of labor services into cash payments, the state domain administrators and lessees must also have depended heavily on their own teams and hired labor. Breysig, *Geschichte der brandenburgischen Finanzen*, 298–99, 359.

⁴¹ On the post-1648 conjunctural trends in the European and German agricultural economies, see Wilhelm Abel, *Agrarkrisen*, 163 and following; and Wilhelm Abel, *Geschichte der deutschen Landwirtschaft vom frühen Mittelalter bis zum 19. Jahrhundert* (Stuttgart, 1962), chaps. 6–7. Despite falling domestic and West European rye prices, Brandenburg producers exported during the years of peace between 1662 and 1674 shipments yielding an average annual customs duty (for nine known years) of over 26,000 Taler; but, in the thirteen years between 1676 and 1688, the grain duty paid an average of only 15,000 Taler. These sums, given the precipitous increase of 1632 in the customs rates and the devaluation of the Taler as a money of account by 14 percent in 1667, suggest that the volume of postwar grain exports was much reduced from pre-1618 levels. From the late seventeenth century until after the Seven Years' War (1756–63), the Junkers were largely dependent on the domestic grain market. Calculations based on data in Rachel, *Handels-, Zoll- und Akzisenpolitik*, 842; see also 32 and following, 82 and following, 208, 238, 269 and following, 511–636; and Naudé, *Die Getreidehandelspolitik*, 60–183. On estate owners' efforts to reduce their dependence on bread grains and expand their brewing and distilling operations, see GStA. Stavenow, no. 282 (*anno* 1694).

If the landlords were to cut their costs of tillage, they needed to resettle the large and small holdings in their villages. This would enable them to return to the pre-war regime of unpaid labor services. It would, by repopulating the countryside, free them of the compulsion to bid up among themselves the price of still scarce wage labor. But it was an expensive undertaking. Not only did the landlords have to grant settlers who were rebuilding fully devastated and abandoned peasant holdings years free of rents and labor services; in many or perhaps most cases, they had to provide the necessary building materials and essential livestock and equipment as well.⁴²

Alternatively, the Junkers could pursue the repopulation of their villages by extra-economic coercion. They could invoke the statutes of 1651 and 1653 to compel the children of their surviving subject farmers, if any there were, to accept and rebuild abandoned farms and thereafter to pay heavy seigneurial rents on them. They could attempt to hold down their wage bills by invoking the letter of the law governing compulsory manorial service. If before the Thirty Years' War they had thought themselves land-poor, they could incorporate abandoned peasant lands into their demesnes and try to evade the government's demands—first, that they pay the taxes due from them and, later, that they should be resettled with peasant families for the sake of the “Peuplierung” of the country and to broaden the pool of military recruits. In short, the landed nobility could try to overcome the unfavorable conditions facing them in the commodity and labor markets by exerting their seigneurial authority to raise the rate of exploitation of their village subjects. To the limited extent that the historical literature has recognized the Junkers' postwar dilemmas as agriculturalists, it has assumed that this was the path they trod.

IN THE SECOND HALF OF THE SEVENTEENTH CENTURY, the nobility and the government did indeed repeatedly attempt to gain their economic ends through

Compare Paul Gottlieb Wöhner, *Steuerverfassung des platten Landes der Kurland Brandenburg*, 3 vols. (Berlin, 1805), 1: 79 and following.

⁴² In 1717 at Stavenow, the value of the labor services and rents in cash and kind of the thirty-six peasant farms resettled since 1649 (most in the 1680s and 1690s) added up, when capitalized at 6 percent, to nearly 6,000 Taler, a large sum included in the price paid that year by the estate's new master; GStA. Stavenow, no. 220. In the Uckermark, the cost to the landlords of the buildings and the essential livestock and tools (*Hofwehr*) of a peasant farm was, around the year 1700, 431 Taler. In 1622, under more favorable market conditions, it had been only 260 Taler; Harnisch, *Die Herrschaft Boitzenburg*, 148–49. In those numerous cases in which the landlords fully financed peasant resettlement (the colonists supplying only their labor), peasant tenurial rights suffered a degradation in comparison with pre-war norms. Outside the districts of strict serfdom, the peasantry typically held such rebuilt farms not as hereditary property (*Erbeigentum*) but in hereditary usage (*Lassitentum*), since the buildings and fixed stock now belonged in law not to the peasant but to his landlord. This, rather than personal enserfment, was the most important legal consequence for the peasantry of the great war. In practice, the difference was not great, since even proprietary peasants were not permitted to alienate or divide among their heirs their farmsteads and fixed stock but rather only, as was also the case among the *lassitische* peasants, other assets and property they had accumulated. See Grossmann, *Über die gutsherrlich-bäuerlichen Verhältnisse*, chap. 4. For examples of postwar peasant settlers with somewhat meager livestock holdings of their own, see GStA. Stavenow, no. 131 (*anno* 1649). Sixteen peasants each held, on average, 2 oxen, 2.33 cows, and 0.67 horses.

judicial-administrative coercion of the peasantry. But, by their own admission, their success was meager, not least because they were competing among themselves for the peasant surplus. The war into which Frederick William plunged in 1655 augured badly for landlord interests under emergent absolutism. Not only did the Elector squeeze more revenue from the land than the agreement of 1653 allowed, but in 1656 he urged the nobility to arm themselves in a feudal levy and to agree to the formation of peasant militias to defend the home front. Their reply was that two-thirds of their brethren were too poor to equip themselves for war. They were already paying heavy taxes “through their subjects, since they had to go without the full measure of labor services and rents the peasants owed them.” The Junkers’ spokesmen feared that the villagers, if armed, would “not only refuse to pay their taxes but will stop rendering their labor services altogether” since, as they added later, the peasants “have become more and more desperate because of so many tax levies.”⁴³

At the war’s end, Frederick William, without having consulted the Estates, decreed “by virtue of princely power and sovereign authority” that all persons prepared to rebuild a devastated and abandoned peasant farm must without exception be granted six years free of taxes, rents, and military quartering. This he justified by the “very ruined” condition of the Mark Brandenburg, whose settlements had been stripped by renewed war “to no small degree of their labor power.” The nobility protested that they had “a high interest in what belonged to them” and that their claims to rents and labor services could not be denied them against their will. They expressed the fear that foreign colonists would abuse their free years to “exhaust the soil” and at their expiration “disappear into the dust.” They should be required to deposit money with their landlords against this eventuality. But the government ignored the Junkers’ complaints and the six-year ruling remained on the statutes.⁴⁴

Between 1660 and the resumption of war in 1674, Frederick William’s regime blamed the slow pace of recovery on Junker egotism and peasant insubordination. In 1667 and again in 1670, Frederick William denounced those among the nobility who, instead of resettling their abandoned peasant farms, annexed the land into their own demesnes or even drove settled peasants from their holdings for this purpose, “as He himself has observed in various places.” The Elector commanded the Estates to desist from such “shameful, pernicious things.” Stung, they replied that the whole nobility should not be blamed for the actions of a few, and that they too wished that all peasant farms were occupied.⁴⁵

At the same time, a decree of 1670 announced that the statute of 1651 on laborers and wages in the Altmark and Mittelmark was “very seldom observed.”

⁴³ Quotations from Isaacsohn, *Urkunden und Aktenstücke*, 10.2: 328, 322, 329; see also Meinardus, *Protokolle und Relationen*, 5: 85–86, 469; 6: 597–99. The nobility’s and his own officials’ protests led Frederick William to issue another edict of 1656 forbidding his recruiters to lure away to the colors the land’s farmers by playing on their dissatisfaction with taxes and military requisitions; CCM, 6.1, no. 215, cols. 491–94. See also Meinardus, 5: 83–84; CCM, 6.1, no. 128, cols. 495–98.

⁴⁴ Frederick William’s edict, CCM, 6.1, no. 131 (1661), cols. 501–02; the nobility’s protests, Meinardus, *Protokolle und Relationen*, 6: 345–46.

⁴⁵ CCM, 5.3.2, no. 3, cols. 335–38; Isaacsohn, *Urkunden und Aktenstücke*, 10.2: 416–17, 424, 535–37.

It complained that “in these times, when still only the fewest villages are repopulated,” the children of the peasantry were refusing to take over empty holdings. Many people in the countryside were living as lodgers from day labor, raising their wages “as they please” and evading “all *oneribus publicis*.” Henceforth, all such persons resident for three years or more in any one place could be compelled to take either a steady job or an abandoned farm. Landlords should try to resist the demand that meals be provided on days of manorial service. But the peasants’ strong bargaining position emerged in the proviso that substitutes should be offered “so that the subjects will stay where they are.”⁴⁶

In the Neumark, “this war-ravaged and devastated land,” the authorities had publicly denounced in 1660 the demand of the “unbridled peasants and workers” that their employers serve them strong beer. These people, who in the war’s aftermath “imagine themselves to be indispensable,” were responsible for the present illegally potent brewing. “One can no longer find a natural beer in the towns, but instead such a thick murky drink that one might think Satan had invented it to smother human reason and drive people deeper into sin and vice.” The government believed that no fewer than fourteen barrels of beer should be brewed from twenty-four bushels of barley. But, in response to the entreaties of the nobility, fearful of the loss of laboring hands, it reluctantly approved a twelve-barrel minimum.⁴⁷

In 1683, there occurred the last assembly of the corporate representatives of the Altmark and Mittelmark nobility before the collapse of the old regime in 1806. The grievances they submitted to the princely regime expressed great bitterness over the swelling power of the absolutist military, fiscal, and judicial administration and the eclipse of their rights of corporate self-administration and consultation with the government. Their only recorded complaint about the peasantry was that “frivolous lawsuits” against their landlords were still not being punished. But otherwise they posed as the agriculturalists’ defender, denouncing strongly the burden on the peasant farmers of direct and indirect taxes, military quartering, grain marketing restrictions, and overpriced manufactures monopolized by the government. Even if their solicitude was genuine, they were conceding at the same time that the absolutist regime had become, to their disadvantage, a major extractor of the peasant surplus.⁴⁸

The corporate nobility’s attacks on the bureaucratized and militarized autocracy did not prevent their political flagship from slipping beneath the waves. But, soon after the assembly of 1683, the government issued a new statute on laborers and wages for the Mittelmark and associated districts, prefaced by the exasperated remarks on the common people’s refractoriness cited at the beginning of this article. Undoubtedly, this law incorporated the suggestions of local estate owners, but in it the government pursued its own objectives, not

⁴⁶ CCM, 5.3.2, no. 5, cols. 337–40. Two years later, the government had to concede that manorial laborers on both Crown and private estates were still compelling their employers illegally to supplement their wages by sharing the seigniorial harvest with them; CCM, 5.3.1, no. 18, cols. 137–38.

⁴⁷ CCM, 6.1, no. 130, cols. 499–502 (quotations, col. 499).

⁴⁸ Isaacsohn, *Urkunden und Aktenstücke*, 10.2: 582–602.

necessarily agreeable to the Junkers. It was, for example, now ordained that no single adults would be tolerated in the villages. The seigneurial authorities would compel everyone, including the farm servants, to marry and settle down by their twentieth year. This, in effect, limited the compulsory manorial service of the children of the *Vollbauern* to those under the age of twenty. More to the nobility's liking was the command that married lodgers must, if they would not or could not take over a farm, render their landlords two days of weekly labor services in exchange for food and drink, a concession at odds with official policy to abolish such meals. Moreover, it was now ruled that all the sons of settled farmers, and not just two, could be compelled to take over unoccupied holdings.⁴⁹

Yet the wage rates decreed in 1683 show that the economic conjuncture still benefited labor. The average of the locally variable, highest permissible yearly cash payments to a fully skilled farm servant or *Ackerknecht* now rose to 12.7 Taler. This represented a nominal increase of 37 percent and a real rise of 32 percent over the rates fixed in 1644. The statute regulated working conditions in the Uckermark for the first time since before the great war. It grimly ratified the Junkers' unlimited powers there over the labor power of those of their subjects trapped in strict serfdom. But their inability to reassemble a sufficiently large population of servile farmers had led them to lease unoccupied holdings in large numbers to short-term tenant farmers who were personally free, a strategy they continued to follow throughout the eighteenth century.⁵⁰

Despite a legacy of strict serfdom, the wages in cash and kind paid to manorial laborers in the Uckermark, a land plagued by "paucity of inhabitants," were higher than elsewhere in Brandenburg. A *grosser Ackerknecht*, capable of working a team of four horses and strong enough himself "to carry six bushels of grain," could each year claim 10 to 13 Taler in cash and the harvest of four to six bushels of grain sown at his landlord's expense, or 4 to 6 Taler in their place. Beyond this, he received housing, fuel, and an ample yearly food allowance. The Uckermark shepherds were commanded to stop demanding as their own every fourth head in the herds they shared with the Junkers and to content themselves with the customary fifth.⁵¹

Two years later, the government issued a revised statute on laborers and wages for the Neumark. It attempted a crackdown on the villagers. Personally free lodgers, if they had lived in one place for four years, were declared hereditary subjects (though not serfs) of their Junkers. Freeman leasing farms

⁴⁹ *Revidirte Pauer-Gesinde-Hirten- und Schäfer-Ordnung* (Mittelmark, Prignitz, Beeskow-Storkow). CCM, 5.3.1, no. 20, cols. 141–70, esp. 142–43, 146–49. The statute also denounced the habit among some settlers of abandoned farms of absconding at the end of their free years.

⁵⁰ Mittelmark wage rates averaged from seven districts, CCM, 5.3.1, no. 20, cols. 150–52, 169–70. On the Uckermark, see cols. 144–45, 152–58, 167–68. On the replacement there of serfs by freemen, see Harnisch, *Die Herrschaft Boitzenburg*, 116 and following.

⁵¹ CCM, 5.3.1, no. 21, cols. 153 and following, 163–64. The 1683 statute entitled threshers in the Uckermark to keep as their own every sixteenth bushel. The statutes of 1644–1645 had tried to limit threshers' shares elsewhere in Brandenburg to no more than the eighteenth bushel. In reality, threshers everywhere appear to have taken a larger proportion of the harvest. The records of church land farmed in the Uckermark in the period 1651–1696 show that threshers' shares varied considerably but in most cases made up at least the fourteenth and in many cases the tenth bushel or more. GStA. Provinz Brandenburg, Rep. 16, no. 95: Kirchenregister von Rossow.

were required to sell the fixed improvements they had carried out to their landlords or else become hereditary subject farmers. The sons of peasants were obliged by the age of twenty-five to take over unoccupied holdings of any size, and not only farms in their fathers' class. Farmers who could not pay their taxes and render their labor services faced demotion to farm-servant status until they had saved enough to take on a new holding. Fierce penalties were spelled out for runaways and those who assisted them. Finally, the wage rates decreed in 1685 either perpetuated those of 1646 or lowered them.⁵²

The reaction of the Neumark villagers to the authorities' efforts to enforce the 1685 statute appears to have been tumultuous. For, in 1687, the government published a "clarification" aimed at "setting a limit to the opposition (*Widersetzlichkeit*)" of the peasantry, farm servants, and rural artisans to the earlier edict, which had stirred up "all manner of conflict and doubts." Even though it did not retreat from its policy of reducing lodgers of four years' duration to legal subjection, the government made considerable improvements in the allowable wages in cash and kind payable to subject and free villagers, male and female, in manorial service and to free and mobile workers in the building and carrying trades. In three counties where the wages paid to *grosse Ackerknechte* can be accurately traced, the money rates fixed in 1687 amounted to an increase since 1646 in nominal terms of 50 percent and in real terms of 43 percent. The clergy may have encouraged the common people's protests by refusing to pronounce the 1685 statute legitimate. The edict of 1687 ordered the local pastors or, if they were too old or weak, their sextons to read both edicts in church without fail, or face stiff penalties.⁵³

The statutes of the 1680s did not reduce the countryside to order. In the Mittelmark, the government complained in 1691 to the church administrators (*Inspectores*) that the statute of 1683 "was observed only in the fewest districts" and that, when disputes came before the *Kammergericht*, people pled ignorance to excuse their violations. The *Inspectores* were commanded to make sure that all pastors read the statute once a year from the pulpit. In 1695, the government had to admit that the law was "almost a dead letter." By 1698, Frederick III's officials, responding to the corporate nobility's complaints about the refusal of the seigneurial authorities to abide by the statute in their judicial and employment practices, had given up on the higher clergy. They now commanded the patrimonial courts—that is, among others, the Junkers themselves—to see that an extract of the 1683 law, presumably of their own formulation, was read annually to the villagers in their churches, an admission by the regime of its inability to force the landed nobility to comply with the law.⁵⁴

At the end of the seventeenth century, local conditions in the labor market

⁵² CCM, 5.3.1, no. 24, cols. 171–212, esp. 171–81.

⁵³ CCM, 5.3.1, no. 26, cols. 213–22, *passim* (quotation, col. 215).

⁵⁴ CCM, 5.3.1, nos. 27–29, cols. 223–24. On the difficulties the absolutist regime faced in enforcing its laws within the jurisdictions of the nobility and at the grass-roots level in general, see Peter-Michael Hahn, "'Absolutistische' Polizeigesetzgebung und ländliche Sozialverfassung," *Jahrbuch für die Geschichte Mittel- und Ostdeutschlands* (Berlin, 1980), 29: 13–29; and Hahn, *Fürstliche Territorialhoheit*, 227 and following.

rather than the government's wage statutes governed the economic relationship between manor and village. In the first two or three decades of the eighteenth century, the restoration of pre-1618 population levels in the countryside strengthened the Junkers' hands in their dealings with their subjects. The landed peasantry, having arduously rebuilt and restocked their farms, ran the risk of losing them by refusing their landlords' long-evaded claims to rents and labor services in full. Farm laborers, increasingly numerous, faced a tighter job market. By the 1730s Berlin was a city of 80,000, where 6,000–8,000 beggars plied their trade.⁵⁵

Under these conditions, it was possible for the authorities, in the statute on laborers and wages of 1722 issued for the Mittelmark and associated districts, to freeze the real wages of freely contracted farm servants at roughly their 1683 levels. Moreover, by raising the age at which villagers should marry from twenty to twenty-five, the government widened the pool from which landlords could forcibly recruit their manorial servants. In a similar statute of 1735, the term of compulsory service in the Altmark, hitherto three years, was extended on the model of the rest of the country until marriage.⁵⁶

These were the reflections in legislative practice of a new and tense era in the landlord-peasant relationship whose discussion need not be pursued here.⁵⁷ But the government's and the corporate nobility's own testimony shows that the first half-century of absolutism did not witness the bending of the rural population's neck under a yoke of seigneurial domination heavier and more irresistible than before the Thirty Years' War. Until the beginning of the eighteenth century, the laws meant to secure the landlords' advantage over their village subjects were widely violated by Junker and peasant alike.

IN THE COURSE OF RESETTLING THEIR VILLAGES with subject farmers, the noble landowners and state domain managers confronted peasants who exploited the scarcity of their own labor in order to resist as long as possible the obligation of rendering their seigneurial dues in full. While the landlords sought the restoration of peasant rents as they had been claimed or paid before the Thirty Years' War, the peasants tried to evade their reimposition.

How far did they succeed in this, and at what cost to their landlords? Full answers are not yet in sight, but the evidence at hand suggests two conclusions about the movement of peasant rents in the seventeenth and early eighteenth centuries.⁵⁸ In nine private and princely seigneurial jurisdictions scattered

⁵⁵ Rachel, *Grosskaufleute*, 2: 4–5; Schultze, *Mark Brandenburg*, 5: 62.

⁵⁶ CCM, 5.3.1, no. 36 (1722), cols. 267–300, esp. 268, 274; no. 39 (1735), cols. 303–32, esp. 311–12.

⁵⁷ See William W. Hagen, "The Junkers' Faithless Servants: Peasant Insubordination and the Breakdown of Serfdom in Brandenburg-Prussia, 1763–1811," in R. J. Evans and W. R. Lee, eds., *The German Peasantry: Conflict and Community in Rural Society from the Eighteenth to the Twentieth Centuries* (London, 1986), 71–101. Compare Hans-Heinrich Müller, *Märkische Landwirtschaft vor den Agrarreformen von 1807* (Potsdam, 1967).

⁵⁸ Peasant rents comprised a complex and varying mix of labor services and haulage, payments in cash and kind, and sometimes (secularized) tithes as well. They cannot accurately be reduced to a

TABLE 3
Average Rents and Labor Services Claimed from Largeholding Peasants
(Vollbauern) in Four Stavenow Villages

	1618–47	1694–1700	1727
1. Days weekly of manorial service with team of horses	3	2.5	2.8
2. Yearly rent in grain (bushels of rye)	7	3.6	7.4

SOURCE:

GStA. Stavenow, nos. 131 (1647), 282 (1694), 356 (1699–1700), 30 (1727).

1618–47: conversion (where necessary) of money rents into rye values based on the average price at Berlin, 1624–53, following Naudé, *Die Getreidhandelspolitik*, 568–69; analogous conversion of 1694–1700 data based on Berlin average, 1653–1702, following Behre, *Geschichte der Statistik*, 277.

throughout Brandenburg and encompassing many dozens of villages, the record of the landlords' formal demands for labor services and shares of the peasants' own production reveals that, in the second half of the seventeenth century, rents of large holders (*Vollbauern*) on rebuilt farms tended to fall below pre-war levels. In the early eighteenth century, the landlords succeeded at best in making good their predecessors' claims of a century earlier. But, in other cases, the postwar rent reductions proved irreversible.⁵⁹

The records of the Stavenow estates illustrate the first of these trends with some accuracy. And, since the seigniorial rents demanded of the peasants there were neither especially heavy nor light in comparison with those of other Brandenburg villagers, their experience may not have been atypical. As Table 3 shows, before the Thirty Years' War, the *Vollbauern* owed their landlords labor services with a team of horses or oxen of three days weekly (apart from one full week's labor in the rye harvest). Beyond this, they delivered to the manor house a yearly average of seven bushels of rye. These were the most important components of their feudal rents. By the years 1694–1700, when most of the abandoned farms at Stavenow had been resettled and their new tenants' free years had expired, the landlords still could not extract from the peasants more than 2.5 days of weekly labor services and half their pre-war grain levies. Even

single quantifiable variable. Nor do formal seigniorial rent claims illuminate daily practice in which, for example, the ruthlessness or reasonableness of a manorial foreman could determine whether labor services were ruinous or merely burdensome.

⁵⁹ For cases in which pre-1626 rents were reduced after the Thirty Years' War but then restored in the late seventeenth or early eighteenth century to pre-war levels: Stavenow (see text below); Wichmann, *Bredtin*, 18–20, 169–73 (eighteenth century: optional low commutation fee in place of labor services); Kiebusch, *Heiligengrabe*, 300–18; and Johannes Simon, "Kloster Heiligengrabe," *Jahrbuch für brandenburgische Kirchengeschichte*, 24 (1929), 3–136, esp. 80–88; Harnisch, *Die Herrschaft Boitzenburg*, 138–39, 206; Vogler, "Badingen," 154 and following; Albrecht, *Die Gutsherrschaft Freyenstein*, 84–112. For cases in which pre-1626 rents fell permanently after the Thirty Years' War, see Carl Brinkmann, *Wustrau: Wirtschafts- und Verfassungsgeschichte eines brandenburgischen Ritterguts* (Leipzig, 1911), 67–68; Passow, *Ein märkischer Rittersitz*, 119 (postwar labor services fixed, peasant farms enlarged); GStA. Provinz Brandenburg, Rep. 37, Gutsherrschaft Kletzke, no. 1 (villages of Kletzke, Kunow, and Viesecke). Data on the Neumark are unavailable, but mid-eighteenth-century rents at Sandow were not unusually high. See Pilsach, "Bäurliche Wirtschaftsverhältnisse," 142–43.

if, as is quite unlikely, the Stavenow Junkers' postwar rent losses had never exceeded these limits, they were still, spread over a period of a half-century and more, considerable. But, in the early eighteenth century, the landlords pressed the Stavenow peasants hard for a return to the old norms. In a settlement of 1727 that retained its force until the emancipation of the early nineteenth century, the peasants had to accept labor-service obligations near the pre-war levels, but they could substitute their actual performance with cash payments (*Dienstgeld*), which most of them chose to do to rid themselves of at least one of their days of weekly manorial service. Grain rents in 1727 stood slightly higher than before the great war but were still in most cases quite low. Moreover, in the settlement of 1727, the peasantry had successfully bargained to reduce their old haulage obligations and persuaded their landlord to lease them seigneurial meadow land on advantageous terms.

It might be objected that, if the Stavenow peasants were obliged finally to re-shoulder the burden of pre-war seigneurial rent, the combined weight on them of these charges and post-1653 absolutist taxation must have staggered them. But, as Table 4 shows, this seems not to have been the case. No doubt princely taxation soared to cruel or unbearable levels during the wars of the 1650s and 1670s and again in the first decade of the eighteenth century. But the government levied the land tax or *Kontribution*, to which by 1733 the peasantry's troop-quartering and other peacetime obligations to the militarized monarchy had been assimilated as cash surcharges, on the basis of a cadastre that considerably under-reported the Stavenow peasantry's tillable and taxable acreage. In 1686, the peasants, probably with their landlords' connivance, succeeded in concealing from the government's tax assessors the full extent of their landholdings. In reality, as the seigneurial rent roll of 1727 shows, they sowed one-third more rye and nearly four times more barley and oats than the government took as the measure of their direct taxes. Fortunately for them, cadastres were difficult and expensive to draw up, and the Prignitz survey of 1686 was not revised before the emancipation of the early nineteenth century.⁶⁰

Thus the share of a Stavenow full peasant's annual rye crop claimed by the *Kontribution* and associated taxes was, at early eighteenth-century average prices, about 16 percent, while seigneurial rent (including *Dienstgeld* for one day's weekly labor service plus the grain levy) consumed about 18 percent. Monetizing the value of a peasant household's combined rye and barley surplus shows that it could, on average, claim 65 percent as its own after paying taxes and feudal rents. Of course, the members of the household consumed much of the surplus

⁶⁰ In the eighteenth century, the peasants at Freyenstein also benefited from the under-reporting of their taxable production. Albrecht, *Die Gutsherrschaft Freyenstein*, 129–31. After 1700, government inquiries uncovered similar evasions in the Crown villages. Rudolph Stadelmann, *Friedrich Wilhelm I. in seiner Thätigkeit für die Landeskultur Preussens* (Leipzig, 1878), 18–19. A memorial to the Crown of 1710 claimed that undertaxation, but also overtaxation, was common throughout the monarchy; Luben von Wullfen, "Relation," in Stadelmann, 214. Wullfen's broadside against official corruption and abuse of the peasantry cautions against breezy views of the period. But the geographical incidence of the conditions he denounces remains unclear. The famine and epidemics that ravaged East Prussia and Pomerania in 1709–1710 scarred Brandenburg as well. Naudé, *Die Getreidehandelspolitik*, 182–83; Stadelmann, 21.

TABLE 4
Grain Output, Rents, and Taxation of an Average Peasant Farm
(Zweihufnerstelle) in Four Stavenow Villages

	1686–1716	1727	1727–33
1. Annual sowings assessed for taxation			
a. barley and oats	7.1 bu.		
b. rye	19.6 bu.		
2. Annual sowings actually cultivated			
a. barley and oats		24 bu.	
b. rye		24 bu.	
3. Rents and taxes as percentages of average rye yields (= 84 bu.)			
a. seigneurial rents			18.2%
b. direct taxes			15.7%
c. seed reserved for next sowing			28.6%
d. farm surplus			37.5%
			100.0%
4. Rents and taxes as percentages of rye and barley surpluses (monetized = 69 Taler)			
a. seigneurial rent			18.3%
b. direct taxes			16.3%
c. consumable and marketable surplus			65.4%
			100.0%

SOURCES:

1. Vogel, *Prignitz-Kataster*, 113–25; Wöhner, *Steuerverfassung*, 2:40–45.
2. GStA. Stavenow, no. 30.
3. Seed-yield ratio of rye = 1:3.5 (GStA. Stavenow, no. 640), of barley = 1:3 (Vogel, *Prignitz-Kataster*, 113–25); seigneurial rent includes commutation of one day of weekly manorial service into *Dienstgeld*; direct taxes include *Kontribution* and *Fourage- und Speisegelder* at 1733–1805 rates (Wöhner, *Steuerverfassung*, 2:71; and GStA. Stavenow, no. 355).
4. Monetization of rye yields at average Berlin price, 1703–1732, following Behre, *Geschichte der Statistik*, 277; of barley at 62.5 percent of rye price (GStA. Stavenow, no. 240).

directly. But grain sales produced only part of their income on the market. They also sold horses and other livestock, spun flax, marketed vegetables and fruit, and earned money from cartage.⁶¹

The economic condition of the Stavenow peasantry cannot have been wildly anomalous. The movement of rent there, as elsewhere in Brandenburg, hardens the point that the emergence of the absolutist regime hindered the landlords from reasserting or raising their pre-war claims on the peasantry precisely during those years, from the 1650s to the early eighteenth century, when the

⁶¹ The data represented in Figure 4 are averages and are not meant to paint the economic situation of the Stavenow peasantry in a rosy light. Years of poor harvests (and war) would have altered the size of their marketable surplus significantly. And, while plague had retreated after the 1680s, other human and animal epidemics still periodically wrought havoc in the villages. The data merely show that the shares of the peasant surplus appropriated on average by taxation and seigneurial rent were not confiscatory.

weight of taxation on the villages was, relatively speaking, the heaviest. Thereafter, the bite of royal taxation was not so deep as to undermine the peasant farm economy in normal years.

BY THE EARLY EIGHTEENTH CENTURY, the noble and Crown estates had recovered from the blows of war. Henceforth, the Junkers' fortunes were tied to the policies of an absolutist regime that regulated, sometimes to their disadvantage, the domestic and export grain and wool trade in its own military and fiscal interest.⁶² The catastrophes of the seventeenth century had changed the face of the Junker class. Numerous families had been ruined by the Thirty Years' War. Many of the pre-war debtors among them had sold their estates at bargain prices to military adventurers enriched by the spoils of war or to well-heeled favorites of the absolutist court.⁶³ Those who before the war had invested their earnings in the public tax corporations were lucky to recover one-third of their capital, shorn of accumulated interest, by the 1670s or 1680s.⁶⁴ Some of the Junkers were able to repair their fortunes by seizure of abandoned peasant holdings.⁶⁵ Others found an economically safe haven in state service, but not as many as the literature sometimes suggests.

Frederick William certainly welcomed the high nobility of his lands, especially its Calvinist members, into his diplomatic and civil service. His generals undoubtedly recruited many of the lesser nobility into the army officer corps. All the same, he relied inordinately on foreign noblemen and bourgeois administrators to launch and sustain the absolutist revolution. In 1689, the number of army officer positions numbered only slightly more than a thousand, of which nearly a third were held by newly arrived refugee Huguenot noblemen. As late as 1720, the number of posts in the higher civil service amounted to only 500 for the entire Prussian monarchy. It must have been a relief to many of the lesser nobility when, after 1713, Frederick William I accorded them preference in

⁶² Naudé, *Die Getreidehandelspolitik*, 199 and following; Rachel, *Handels-, Zoll- und Akzisepolitik*, 648–753; Carl Hinrichs, *Die Wollindustrie in Preussen unter Friedrich Wilhelm I* (Berlin, 1933), 133 and following, 377–78; Karl Heinrich Kaufhold, "Leistungen und Grenzen der Staatswirtschaft," in Schlenke, ed., *Preussen*, 106–19.

⁶³ See the *Landes-Recess* 1653 (Neumark), col. 472; Schultze, *Mark Brandenburg*, 5: 305; Ulrich Wille, *Die ländliche Bevölkerung des Osthavellandes vom Dreissigjährigen Krieg bis zur Bauernbefreiung* (Berlin, 1937), 60–64; Rachel, *Grosskaufleute*, 2: 11–22, 102–12.

⁶⁴ Isaacsohn, *Urkunden und Aktenstücke*, 10.2: chap. 4. Rachel, *Grosskaufleute*, 1: 379–91; Hahn, *Fürstliche Territorialhoheit*, 201–02.

⁶⁵ In the Mittelmark, farmland equivalent to 573 large peasant holdings (*Zweihufnerstellen*) was engrossed into the large-estate sector between the end of the Thirty Years' War and the early eighteenth century, comprising some 25 percent of all domanial land in its boundaries of the year 1800. Siegfried Korth, "Die Entstehung und Entwicklung des ostdeutschen Grossgrundbesitzes," *Jahrbuch der Albertus Universität zu Königsberg*(Pr., 3 (1953): 162; the analogous figures for the Uckermark are 420 *Hufen*, amounting to 22.5 percent of the estate land in the year 1800. According to Grossmann's figures (*Über die gutherrlich-bäuerlichen Verhältnisse*, 71), the number (not area) of full peasant holdings (*Bauernstellen*) in the Mittelmark in 1725 was 10 percent lower than in 1624, but in 1750 it was 2 percent higher. Analogous figures for cottagers: –42 percent, +10 percent; for laborers without arable land (*Hausleute*): +100 percent; +300 percent. On Frederick William I's retreat from the policy requiring the resettlement of all peasant holdings in their pre-war form, see CCM, 5.3.2, nos. 20 (1709), cols. 359–62, and 24 (1717), cols. 363–66.

appointments to an expanding officer corps of an army that, during his reign, never fought a war.⁶⁶ Nonetheless, in the late eighteenth century, when the Junkers are sometimes thought to have returned, with Frederick II's blessings, to the center of the political stage, only some 15 percent of the nobility equipped themselves with a military education, while no more than that number acquired the credentials for a career in the civil service. Two-thirds of the late eighteenth-century Brandenburg-Prussian nobility had no prospects for employment in the state administration supposedly raised up for their benefit as a class.⁶⁷

It seems highly probable that, among the two-thirds of the 259 noble families of Brandenburg that died out or disappeared between 1540 and the late eighteenth century, a majority succumbed between the Thirty Years' War and the early eighteenth century. In any case, under the regime of absolutism, the casualties were replaced primarily by rising members of the military and bureaucratic nobility, including immigrant noblemen and ennobled commoners.⁶⁸ The regime of absolutism certainly depended, in addition to bourgeois talent, on a service nobility. But the historical literature, especially when it rests on the conviction that the "compromise of 1653" guaranteed the vital economic interests of the noble class, overdraws the continuity between its eighteenth-century and pre-absolutist forms. The nobility of the eighteenth century was, in important respects, a new class, many of whose members were indispensable both as state servants and landlords to the monarchy. But they were compelled to compete among themselves and with the educated and propertied commoners for state posts, while absolutist mercantilism and militarism constrained their freedom of action in the economic tug of war with their village subjects.

One of the principal aims of early absolutism in Brandenburg was, undoubtedly, the reestablishment of the agrarian regime, shaken and in part dissolved by the Thirty Years' War, of noble estates and state domains worked by a subject peasantry. But Frederick William and his successors did not purchase the Junkers' assent to their state-building innovations by sanctioning intensified noble domination of the villages. The ascendant absolutist regime had bills of its own to present to the peasant farmers, who in their turn played taxes against seigneurial rents in such a way as to compel their landlords to bear a significant share of the costs of the bureaucratized state.

In Brandenburg, the seventeenth-century crisis did not erupt from an impending structural breakdown of the social and political system that had

⁶⁶ Hahn, "Landestaats," 63 and following; Hahn, *Fürstliche Territorialhoheit*, 269 and following. Compare Gustav Schmoller, "Über Behördenorganisation, Amtswesen und Beamtentum im Allgemeinen und speciell in Deutschland und Preussen bis zum Jahre 1713," = *Acta Borussica: Die Behördenorganisation und die allgemeine Staatsverwaltung Preussens im 18. Jahrhundert* (Berlin, 1894), 1: 79–143. On the size and membership of the army officer corps in 1689, Carsten, *Origins*, 272; and Rosenberg, *Bureaucracy*, 59; on the size of the higher civil bureaucracy in the eighteenth century, Baumgart, "Wie absolut war der preussische Absolutismus?" 102; Wehler, *Deutsche Gesellschaftsgeschichte*, 1: 261–63. Note also that, after 1679, the government was no longer inclined or compelled to treat the state domains as noble sinecures but leased them instead to bourgeois tenant farmers.

⁶⁷ Heinrich, *Adel*, 308. See also Fritz Martiny, *Die Adelsfrage in Preussen vor 1806 als politisches und soziales Problem* (Stuttgart, 1938).

⁶⁸ Heinrich, *Adel*, 305–06, and 299–312, *passim*.

arisen during the sixteenth century. Whatever the strains gripping the land in the early seventeenth century, it was only the bloodying it suffered in the Thirty Years' War that triggered the crisis, whose causes were more political than socioeconomic. For it is quite thinkable that the pre-war political regime could have been restored after 1648, as happened in neighboring Mecklenburg to the advantage of its own anti-absolutist Junkers. But, in Brandenburg, Frederick William's ambition to create a political regime uniting the electoral heartland with the Hohenzollerns' territorial acquisitions in East Prussia and on the Rhine produced a decades-long postwar crisis of distribution, arising out of taxation and war. This was accompanied by a crisis of production in the second half of the seventeenth century, not only because the terms of trade had shifted against agriculture east of the Elbe but equally or even more so because harsh taxation and other military imposts in the villages and towns of Brandenburg impeded their demographic and economic recovery. Between 1679 and 1713, the exactions of the state diminished enough to bring an end to the production crisis. In the following decades, the absolutist regime stabilized its fiscal levies and laid out the mercantilist grid within which the economic development of Brandenburg-Prussia proceeded for the rest of the old regime.

The seventeenth-century crisis in Brandenburg was in a double sense state-induced: the military weakness of the sixteenth-century regime had invited the ravages of the great war, while the exactions of its successor enfeebled the social organism long after 1648. The peasants and burghers of Brandenburg paid the price both of the state's weakness and its gathering strength. But so, too, did the Junkers. Their coercive powers over the villages failed them during the drastic labor shortage into which they were plunged by the Thirty Years' War and Frederick William's subsequent trials of arms. When the smoke of the seventeenth-century crisis had cleared, they confronted a subject peasantry whose labors for the king of Prussia left them unwilling to follow unaccustomed commands from the manor house.

The Inquisitors of Languedoc and the Medieval Technology of Power

JAMES GIVEN

THE TWELFTH AND THIRTEENTH CENTURIES were a period of major development in the political institutions of Western Europe.¹ In many places, relatively large-scale kingdoms and principalities exercising a degree of authority over millions of subjects came into existence. The directors of European society equipped themselves with institutions of governance that were increasingly professionalized in their staffs and bureaucratized in their operations. Everywhere, jurists and learned writers bent their efforts to the rationalization of legal systems and the elaboration of ideological justifications for the existing distribution of wealth and authority.

In short, the question of political power—its nature, its extent, and the ways in which it was wielded—became one of the central issues of the day. Historians have long been interested in these aspects of power, and, thanks to their labors, we possess an impressive body of scholarship dealing with political thought, administrative history, and the detailed narrative of political events. The question, however, of what “power” meant in this society, how it was acquired, and how it was wielded, has been slighted. Implicit in much of the scholarship is the belief that power was an effect of the structures of medieval society, that it was something that almost automatically accrued either to those classes that were economically dominant or to the masters of specialized political institutions. Among most medievalists, the dominant interpretive schemes suggest that political power was concentrated and effectively organized primarily through the creation of specialized, permanent institutions of governance. The kings and princes who fashioned these institutions were able, thanks to the greater “rationality” and efficiency of these new techniques of governance, not only to triumph over their rivals but to create state organizations that benefited society as a whole by providing more peace and security and “more opportunity for the good life” than the loose associations of communities that had preceded them.² To adopt this line of reasoning, however, is both to exaggerate the effectiveness

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² For a classic statement of this line of reasoning, see Joseph R. Strayer, *On the Medieval Origins of the Modern State* (Princeton, N.J., 1970). The quotation is from page 10.

of medieval governing institutions and to fall into the trap of adopting as an analytic strategy the corporatist ideology of the rulers of medieval Europe.

Marxists, of course, are not prone to interpret the rise of political power as the result of the creation of bureaucratic institutions better capable of promoting the interests of society as a whole. But the powerful analytic traditions that cluster under the rubric of Marxism have frequently failed to provide an adequate account of political power. Some Marxists have not progressed much beyond Marx and Engels's combative observation that the state is no more than a committee for the handling of the ruling class's common affairs.³ Some, especially those of a "structuralist" bent, influenced by their desire to see people as no more than the supports of the structures of their societies, whose every action is the determined effect of those structures,⁴ have argued that political power is something that is simply distributed by the all-powerful and all-determining "combinatory" of a society's structures.⁵

Neither of these approaches is adequate. Political power is not automatically produced by the structures of a society, it is above all a set of relations between human beings. Power is not something that exists in the interstices of the social structure, nor is it an appendage of bureaucratization or rationalization; rather it is the product of the interaction of men and women who seek to dominate, control, and resist one another. Power is a set of concrete practices that must be continually renewed, rethought, and reapplied in the changing circumstances of history. Those who occupy a position of economic dominance within society may be in a better position than others to secure political power, but to do so they must actively struggle with and against others in the arena of politics.

In understanding the steps medieval politicians took to win power for themselves, it may be helpful to think of political activity as a form of production. Like all other forms of production, political activity is a labor process in which raw materials are transformed into a new commodity. Would-be political leaders must take people as they find them, organized in various competing and cooperating social groups, and transform them, through the application of those economic and cultural resources their society offers, into organized groups that can produce the desired commodity, the realization of a policy goal.⁶ In effect, political activity can be understood as a form of technology, as a body of specific techniques for manipulating social relations.

If this line of reasoning seems too Marxist, the reader might prefer a game-playing analogy. The anthropologist F. G. Bailey has likened politics to an arena in which various games are played. These games are governed by rules,

³ "The executive of the modern State is but a committee for managing the common affairs of the whole bourgeoisie"; "Manifesto of the Communist Party," in Robert C. Tucker, ed., *The Marx-Engels Reader*, 2d edn. (New York, 1978), 475.

⁴ Louis Althusser and Etienne Balibar, *Reading Capital* (London, 1970), 252. For a critique of this position, see E. P. Thompson, *The Poverty of Theory and Other Essays* (New York, 1978).

⁵ This would seem to be an appropriate reading of the difficult ideas and less-than-transparent prose of Nicos Poulantzas, *Classes in Contemporary Capitalism* (London, 1975), 13–35. See also Martin Carnoy, *The State and Political Theory* (Princeton, N.J., 1984), 89–127.

⁶ My discussion here leans on that of Barry Hindess and Paul Q. Hirst in *Pre-Capitalist Modes of Production* (London, 1975), 36–37.

some normative and explicit, others implicit and pragmatic. The normative rules define the prizes, the nature of the teams, and what behavior is acceptable or unacceptable. What I have referred to as the technology of power can be understood as the pragmatic rules, a set of unspoken, occasionally secret, prescriptions about how to play the game, not in a normatively acceptable way but how to play it to win.⁷

Thanks to the labors of generations of medievalists, we know a great deal about the structure of formal political institutions in the central Middle Ages, about the methods of political organization and about the ways in which orders were drawn up and transmitted. We know, unfortunately, much less about how the agents of these institutions sought to fulfill their duties and impose their will on the often refractory people whom they ostensibly governed. By surveying the work of the inquisitorial tribunals of Languedoc, I will examine what can be regarded, depending on which analogy is preferred, as either the technology the inquisitors fashioned to help them stamp out heresy or the pragmatic rules they followed in order to win at the deadly serious game they played.

The Inquisition was a response to the spread of heretical belief and practice in twelfth and early thirteenth-century Europe. One of the areas where heresy presented an important challenge to the church was Languedoc in the south of France. At the beginning of the thirteenth century, this region, though nominally subject to the king of France, was virtually autonomous.⁸ During the twelfth century, the dualist heresy known as Catharism managed to acquire an important following. (Waldensianism, although apparently not as important, also gained a foothold in the country.) When the local rulers failed to root out the heretics, Pope Innocent III called on the nobles of northern France to drive out the native lords and deal with the problem themselves. The Albigensian Crusades, which lasted from 1209 to 1229, ended Languedocian independence and saw the area brought directly under royal rule, but they did not end heresy. Heretics, especially the Cathar *Bonshommes*, the men and women who had received the *consolamentum*, the baptism of the holy spirit that cleansed one of sin and guaranteed escape from the cycle of reincarnation in the prison of the flesh, simply went underground.

The church authorities therefore found themselves in an embarrassing situation. The church's legislation on heresy and the punishments appropriate for it were widely scattered and unsystematic. The task of pursuing heretics had traditionally belonged to the local episcopate, which in Languedoc was at first either incapable or unwilling to attack the problem. Even after the *Bonshommes* had been forced underground, they retained the favor of a segment of the population, which was willing to aid and support them against the authorities. Furthermore, the churchmen had only a foggy idea about what heretics believed and did. All this made it hard to identify, arrest, and prosecute them.

⁷ F. G. Bailey, *Stratagems and Spoils: A Social Anthropology of Politics* (Oxford, 1969), 3–7.

⁸ For a brief, but useful, sketch of the political situation of Languedoc on the eve of the Albigensian Crusades, see Charles Higounet, "Problèmes du Midi au temps de Philippe Auguste," in *La France de Philippe Auguste: Le Temps des mutations*, ed. Robert-Henri Bautier (Paris, 1982), 311–20.

In the late 1220s and 1230s, the papacy began to address these problems systematically. In 1231, Pope Gregory IX codified the existing legislation concerning heresy.⁹ More important, he created the Inquisition as an institution. This, however, is a statement that requires qualification. In the Middle Ages, there was no single Inquisition, with a Grand Inquisitor supervising the holy office from Rome or Avignon. Instead, a number of inquisitorial tribunals, some run by Franciscans or Dominicans, others by local bishops, were scattered across Europe. In the Middle Ages, these were never integrated into one unified organization; indeed, virtually no provision was made to assure mutual cooperation between them.¹⁰

Despite the lack of institutional definition and coordination, all medieval inquisitorial tribunals shared certain characteristics. They possessed an extraordinary jurisdiction. Tribunals staffed by members of the mendicant orders were usually exempt from ordinary episcopal control and were immediately subject to the pope. The inquisitors proceeded against suspected heretics *ex officio*, by virtue of their office, without having to wait for a formal accusation to be brought against a suspect. Those who appeared before the inquisitors were made to take an oath and testify against themselves under pain of prosecution for either contumacy or perjury. All proceedings were secret. Moreover, the accused were denied the assistance of attorneys or notaries. They were not told the names of those who had testified against them. Depositions against suspects were also accepted from types of witnesses normally barred by canon law from giving testimony: children, convicted criminals, accomplices, and other heretics. Finally, a condemned prisoner had no right of appeal from the decisions of the inquisitors.

Some of the best-known inquisitorial tribunals functioned in Languedoc, where Gregory turned the task of prosecuting heretics over to the Dominicans. Eventually, these friars established two tribunals, one at Toulouse, the other at Carcassonne. On occasion, their work was supplemented by episcopal tribunals, which functioned from time to time in Carcassonne, Albi, and Pamiers. The churchmen who held office as inquisitors in Languedoc were primarily responsible for working out inquisitorial procedures as they were understood in the Middle Ages. They also experienced one of the greatest triumphs of the medieval Inquisition, the virtual eradication of Catharism in Languedoc by the

⁹ The Inquisition has long been a favorite topic of medievalists. Among many works, one may consult Jean Guiraud, *Histoire de l'inquisition au moyen âge*, 2 vols. (Paris, 1935–38); Johann Joseph Ignaz von Döllinger, *Beiträge zur Sektengeschichte des Mittelalters*, 2 vols. (Munich, 1890); Yves Dossat, *Les Crises de l'inquisition toulousaine aux XIII^e siècle (1233–1273)* (Bordeaux, 1959); Elie Griffé, *Le Languedoc cathare et l'Inquisition (1229–1329)* (Paris, 1980); Charles Molinier, *L'Inquisition dans le Midi de la France au XIII^e et au XIV^e siècle: Etude sur les sources de son histoire* (Paris, 1880); Henri Maisonneuve, *Etudes sur les origines de l'inquisition*, 2d edn. (Paris, 1960); Henry Charles Lea, *A History of the Inquisition of the Middle Ages*, 3 vols. (New York, 1955); L. Tanon, *Histoire des tribunaux de l'inquisition en France* (Paris, 1893); Richard Wilder Emery, *Heresy and Inquisition in Narbonne* (New York, 1941); Lothar Kolmer, *Ad capiendas vulpes: Die Ketzerbekämpfung in Südfrankreich in der ersten Hälfte des 13. Jahrhunderts und die Ausbildung des Inquisitionsverfahrens* (Bonn, 1982); and Edward Peters, *Inquisition* (New York, 1988).

¹⁰ On this, see Richard Kieckhefer, *Repression of Heresy in Medieval Germany* (Philadelphia, 1979), 4–6.

mid-fourteenth century,¹¹ as well as some of the greatest reversals, among them the assassination of several inquisitors at Avignonet in 1242. Finally, the Languedocian inquisitors left behind them one of the largest bodies of records from any medieval inquisition. The greater part of the following remarks will be based on the experience of the Languedocian inquisitors.

THE SCOPE OF DOMINICAN ACTIVITY was often truly impressive. In the early, heroic days of the late 1230s and early 1240s, they organized massive hunts for heretics. Once they had decided that a region was infected with heresy, they undertook a tour of inspection. When they arrived in a town, they assembled the population and preached a sermon setting forth the purpose of their visit. To entice those with knowledge of heretical activities, the inquisitors declared a grace period during which those who came forward with confessions were assured of light penalties. At the end of this period, the inquisitors began the business of summoning and interrogating suspects. A fragment of the investigation carried out by Bernard de Caux in 1245 and 1246 in the diocese of Toulouse has survived. This fragment concerns only the two archdeaconries of Lanta and Vieilmorez. From this relatively small region, Bernard summoned at least 5,471 individuals to testify.¹² Often, entire villages were required to appear before the inquisitors. At least 420 people from the village of Le Mas-Saintes-Puelles were interrogated by Bernard and his assistants.

In the late thirteenth and early fourteenth centuries, such large-scale inquiries became rarer, although they did not cease altogether. In part, this was a result of the growing scarcity of heretics. In part, it was because, as the inquisitors built up dossiers about suspects, they were able to take a more precise and surgical approach to their task. Usually, the inquisitors summoned only a few individuals at a time; however, the number of people who passed through their hands remained large. The book of sentences compiled by Bernard Gui, the inquisitor of Toulouse, indicates that between 1308 and 1323 he condemned 930 people for heresy.¹³ This figure undoubtedly represents only a fraction of those suspects and witnesses interviewed. In 1308, Geoffroy d'Ablis arrested the entire population of Montailhou in the county of Foix. After a preliminary sorting out was completed in the local castle, a large number of the village's inhabitants were carted off to Carcassonne for further investigation.¹⁴

¹¹ The eradication of Catharism was not, of course, solely the work of the inquisitors. For some of the other factors that helped the church authorities in their struggle against heresy in Languedoc, see Jean-Louis Biget, "L'Extinction du catharisme urbain: Les Points chauds de la répression," *Cahiers de Fanjeaux*, 20 (1985): 324–32.

¹² Dossat, *Crises*, 232.

¹³ C. Douais. *Documents pour servir à l'histoire de l'inquisition dans le Languedoc*, 2 vols. (Paris, 1900), 1: ccv. Bernard Gui's register was published by Philipp van Limborch in his *Historia inquisitionis, cui subjungitur liber sententiarum inquisitionis Tholosanae ab anno Christi MCCCVII ad annum MCCCXIII* (Amsterdam, 1692). Most historians of the Inquisition have believed that the original from which Limborch prepared his edition was lost. Gui's register, however, has survived and is today housed in the British Library as Add. MS. 4697. See M. A. E. Nickson, "Locke and the Inquisition of Toulouse," *British Museum Quarterly*, 36 (1971–72): 83–92.

¹⁴ On this incident, see Griffe, *Languedoc cathare*, 262–64.

The inquisitorial tribunals were above all judicial organizations. At first glance, it might seem self-evident that the directors of the church would turn to a judicial organization to handle the task of repressing heresy. But the creation of a special jurisdiction over heresy came only after a long series of experiments with other methods of combating heterodoxy—preaching, the creation of a mendicant order whose members self-consciously imitated the style of life of the Cathar *Bonshommes*, and organized, large-scale violence.

The creation of inquisitorial procedures should be understood as yet another example of the growing use across Europe of judicial mechanisms as one of the primary methods by which political authority could be both asserted and legitimated in the developing polities of the high Middle Ages. It is hardly surprising that medieval rulers should make intensive use of judicial mechanisms as one of their primary devices for penetrating into the heart of the social life of the communities over which they claimed leadership. The intensive study of law that was a prominent feature of European culture in the twelfth and thirteenth centuries allowed rulers to recruit an ever-more-skillful and accomplished set of jurists into their service. Royal and seigneurial intervention into local affairs could, if undertaken under a judicial guise, be clothed in an ideologically acceptable, indeed, attractive, garb. Finally, such intervention often addressed the needs of at least a segment of the local population.

Whether or not one wishes to accept this argument, it is clear that in the twelfth and thirteenth centuries a major movement was under way in which older judicial mechanisms were being replaced by courts whose procedures were not merely new but far more authoritarian. Before the twelfth century, courts had been more the expression of the will of local communities assembled to do justice than expressions of the controlling aspirations of rulers. Directed by judges who functioned as their courts' presidents, judicial tribunals had been more theaters for the negotiation of settlements based on compromise than the sites of definitive judgments authoritatively imposed on litigants. Actions had normally been initiated by private petitioners, not by public authority, and the modes of proof employed—ordeals, recitation of oaths to help the accused, battle—were ones beyond the control of judges.

In the twelfth and thirteenth centuries, however, courts became the agents of the rulers of Western society. A better trained and more professionalized judiciary came into existence, modes of proof more susceptible to their control were devised,¹⁵ and a more activist and interventionist role was adopted by the

¹⁵ For example, in French royal courts by the end of the thirteenth century, civil cases were being settled not by appeal to such modes of proof as trial by battle but by the examination and weighing by judges of written evidence. For a brief description of the procedures of the French royal court at the end of the thirteenth century, see Ferdinand Lot and Robert Fawtier, *Histoire des institutions françaises au moyen âge*, vol. 2 (Paris, 1958), 386–89. It should be noted that Robert Bartlett has recently argued that the unilateral ordeal, frequently interpreted as a “therapeutic” and “popular” form of proof used by small communities that valued consensus and saw in the judicial process primarily a means to restore peace to the community, was in actuality a form for the assertion of royal or princely authority over the judicial process. See his *Trial by Fire and Water: The Medieval Judicial Ordeal* (Oxford, 1986), esp. 34–42.

courts.¹⁶ While courts in the earlier Middle Ages had primarily been institutions for small, face-to-face communities to repair injuries to relationships and fashion a workable peace that would allow the community to continue to function,¹⁷ by the period with which we are concerned they were well on their way to being places where judges imposed definitive verdicts that accorded less with community sentiments and more with conceptions of justice and legality that were the products of a learned, elite culture.¹⁸

It is against this background that the development of inquisitorial techniques should be understood. In the earlier Middle Ages, criminal cases had proceeded by means of the *accusatio*, derived from Roman law. Such an action could only be brought by the victims, their kinspeople, or their lords. In this procedure, the courts and judges played a largely passive role. Burden of proof was borne by the accusers who, if their suit failed, were subject to the same penalties the accused would have had inflicted on them had they been condemned. The accused could clear themselves by means of the ordeal, by producing friends to recite oaths, or offering to fight duels with their accusers. In the twelfth century, the church courts began to develop procedures that allowed them an active role in the prosecution of crime. Ecclesiastical judges claimed the right to proceed *ex officio* against perpetrators of notorious crimes. A procedure of *denunciatio* in which someone could accuse someone else of crime without taking on the awful burdens and risks entailed by *accusatio* was also developed; in these cases, judges proceeded *ex officio* and as if there were no accuser, to require suspects to purge themselves by oath or ordeal. By the end of the twelfth century, the procedure of *inquisitio* had been introduced into church courts, allowing judges to initiate action against those suspected of crime solely on the basis of public rumor. In prosecuting such suspects, judges could take an active role, seeking witnesses or other proof of their guilt. Procedure by *inquisitio* was quickly adopted by lay jurisdictions, including the cities of northern Italy and the French monarchy.¹⁹ By the second half of the thirteenth century, Louis IX's judges were making use

¹⁶ A classic example of this is the development in England of the presenting jury, a device that allowed public authorities to prosecute crime without relying on the cumbrous and, for the accuser, perilous appeal by a private party. For a recent discussion, see Thomas Andrew Green, *Verdict According to Conscience: Perspectives on the English Criminal Trial Jury, 1200–1800* (Chicago, 1985), 3–27. On the greater activism of courts and judges in thirteenth-century Siena, see Peter Raymond Pazzaglini, *The Criminal Ban of the Sienese Commune, 1225–1310* (Milan, 1979), 102–03.

¹⁷ This is basically the argument of Peter Brown, "Society and the Supernatural: A Medieval Change," in *Society and the Holy in Late Antiquity* (Berkeley, Calif., 1982), 310–14; and Paul R. Hyams, "Trial by Ordeal: The Key to Proof in the Early Common Law," in Morris S. Arnold, et al., eds., *On the Laws and Customs of England: Essays in Honor of Samuel E. Thorne* (Chapel Hill, N.C., 1981), 95–97.

¹⁸ The potential for conflict in this situation is illustrated by what took place in northern Wales after its conquest by the English in the late thirteenth century. In the new royal Principality of North Wales, the English Crown abolished Welsh criminal law, which had largely treated crime as a matter to be settled by negotiation and the payment of money compensation by one kindred to another, and substituted for it its own draconian system, which punished infringements of the king's peace with execution, mutilation, and, to a lesser extent, imprisonment. This was an innovation that seemed, at least to some in Wales, to be a change for the worse. See James Conway Davies, "Felony in Edwardian Wales," *Transactions of the Honourable Society of Cymmrodorion* (1916–17): 145–96; and Gwyn Williams, trans., *Welsh Poems: Sixth Century to 1600* (Berkeley, Calif., 1974), 74.

¹⁹ For a thirteenth-century jurist's discussion of the *inquisitio* procedure in criminal trials, see Albertus Gandinus, "Tractatus de maleficiis," in Hermann U. Kantorowicz, *Albertus Gandinus und das Strafrecht der Scholastik*, 2 vols. (Berlin, 1907–26), 2: 37–48.

of procedures modeled on those of the church in the trying of criminal cases.²⁰

When the papacy decided to entrust the task of forcible repression of heresy to a special judicial tribunal on the basis of inquisitorial procedures, it was thus drawing on a body of techniques that were being used increasingly widely in thirteenth and fourteenth-century Europe. The men who staffed these inquisitorial tribunals in Languedoc made the most of their opportunities and made their tribunals a redoubtable foe of heresy in the south of France.

IN FASHIONING A BODY OF TECHNIQUES for manipulating the objects of their inquiries, the inquisitors took few steps that can be regarded as completely innovative, but the way in which they incorporated existing techniques of rule into a peculiarly coherent and effective engine of repression is possibly without parallel in medieval Europe. One of the more novel aspects of the inquisitors' work was the use they made of imprisonment. Imprisonment was not commonly employed in medieval Europe as either a punishment or a means of rehabilitation. In most legal systems, the predominant purpose of imprisonment was to keep someone on hand until trial. Time in prison was for the most part socially and juridically formless, a dead time spent in a dead space between arrest and trial or between trial and execution.

In the thirteenth and fourteenth centuries, however, judges experimented with imprisonment as a form of punishment. Ecclesiastical judges, forbidden by canon law from shedding blood, were particularly interested in developing the notion that imprisonment could be used as a form of punishment. Moreover, bishoprics and monasteries had traditionally maintained prisons and had disciplined errant clerics with incarceration. When Pope Boniface VIII in 1298 declared that imprisonment was an appropriate form of punishment for clerics, he was not engaged in a radical break with past tradition.²¹ The records of papal courts sitting in Avignon in the fourteenth century indicate that the pope's judges made some use of imprisonment as a form of punishment.²² Secular jurisdictions in the thirteenth century were also experimenting with the use of imprisonment, either to force suspects to submit to trial by inquest or as a form of punishment.²³

²⁰ Material in this paragraph is derived from A. Esmein, *A History of Continental Criminal Procedure, with Special Reference to France* (Boston, 1913); and the article by Fredric L. Cheyette, "Inquest, Canonical and French," in the *Dictionary of the Middle Ages*, vol. 6 (New York, 1985), 478–80.

²¹ *Corpus iuris canonici*, ed. E. Friedberg, 2 vols. (Graz, 1959). Liber Sextus 5.9.3.

²² Jacques Chiffolleau, *Les Justices du Pape: Délinquance et criminalité dans la région d'Avignon au quatorzième siècle* (Paris, 1984), 226–32.

²³ Annik Porteau-Bitker, "L'Emprisonnement dans le droit laïque du moyen âge," *Revue Historique de Droit Français et Étranger*, 4th ser., 46 (1968): 389–409; Esmein, *History of Continental Criminal Procedure*, 72; Christopher Harding, et al., *Imprisonment in England and Wales: A Concise History* (London, 1985), 7–19; Ralph B. Pugh, *Imprisonment in Medieval England* (Cambridge, 1968), 18–21, 26–41. See also the remarks in Bronislaw Geremek, *The Margins of Society in Medieval Paris* (Cambridge, 1987), 16–18. It should be noted that, although imprisonment may have been increasingly prescribed as a form of punishment, it may have been rarely applied. Although in England the statutes of Westminster I (1275) and Westminster II (1285) laid down terms of imprisonment as punishments for the abduction of women, convicted ravishers were probably imprisoned, not to punish them but to coerce them into more rapidly paying the damages and

The inquisitors, in using imprisonment to manipulate and coerce heretics, were therefore working with a technique that was receiving increasing attention from their contemporaries. The inquisitors, however, seem to have been far more purposeful in their use of imprisonment than other thirteenth and fourteenth-century authorities.²⁴ Not only was imprisonment for life or a term of years one of the inquisitors' favorite punishments, it was probably their most effective technique of interrogation. In the early years of inquisitorial activity in Languedoc, torture was only rarely used to extract confessions from suspects.²⁵ Instead, uncooperative witnesses were simply locked away for long periods to think things over. Bernard Gui observed that an imprisonment of several years frequently persuaded even the most obdurate of suspects to confess.²⁶ Guillem Salavert of Albi, for example, was first interrogated concerning his heretical beliefs on February 24, 1300. But it was only after he had spent nineteen years continuously under lock and key that his trial was concluded and sentence imposed on him on September 30, 1319.²⁷

The inquisitors were confident that prolonged incarceration of suspects could successfully influence the behavior even of those who were not actually their prisoners. Bernard Gui counseled his fellow inquisitors that protracted detention could be very useful in the trials of Cathar *Bonshommes* (usually referred to by the inquisitors as *perfecti*). Long periods spent lodged in the inquisitors' jails might lead *Bonshommes* to convert to orthodoxy and thereby give the inquisitors a major victory. But, even if *perfecti* steadfastly refused to confess, keeping them prisoner instead of sending them straight to the stake could have some benefits. As time dragged on, so Bernard noted, a *Bonhomme's* associates began to fear that the prisoner would eventually break down, confess, and denounce them. This fear often induced the associates to come forward with confessions. Even if they did not voluntarily confess but had to be cited to appear, their fears made it easy to extract the full truth.²⁸

To encourage confessions, the inquisitors often made the conditions under which suspects were held deliberately severe. In his *Practica*, Bernard Gui advised his fellow inquisitors that they could loosen suspects' tongues by keeping

amercements they owed the Crown and their victims. See Sue Sheridan Walker, "Punishing Convicted Ravishers: Statutory Strictures and Actual Practice in Thirteenth- and Fourteenth-Century England," *Journal of Medieval History*, 13 (1987): 237–50.

²⁴ The inquisitors' prisons often contained large numbers of detainees. A roll of accounts reveals that in January 1256 there were 219 adults and eighteen infants lodged in the prisons of the inquisition of Toulouse; Edmond Cabié, "Comptes des inquisiteurs des diocèses de Toulouse, d'Albi & de Cahors, 1255–1256," *Revue Historique, Scientifique et Littéraire du Département du Tarn* (1905), 217.

²⁵ In medieval legal proceedings, torture was resorted to in the effort to discover a "full" proof of a suspect's guilt. A full proof required the testimony of two eyewitnesses, the apprehension of the suspect in the act of committing the crime, or a confession. All other forms of evidence constituted only a partial proof. If the judge felt that he had acquired sufficient partial proofs to indicate that the guilt of the suspect was likely, he could endeavor to obtain a full proof by resorting to torture. On this, see Edward Peters, *Torture* (Oxford, 1985), 54–58.

²⁶ Bernard Gui, *Manuel de l'inquisiteur*, 2 vols., ed. and trans. G. Mollat (Paris, 1926–27), 2: 56–57.

²⁷ Molinier, *L'Inquisition dans le Midi*, 99 n. 3. See also 330–31 for other cases of suspects whose trials occupied several years.

²⁸ Bernard Gui, *Practica inquisitionis heretice pravitatis*, ed. C. Douais (Paris, 1886), 218; Gui, *Manuel*, 1: 16–17.

them in chains and restricting their food.²⁹ When, in 1306, two cardinals who were charged by Pope Clement V with the investigation of complaints from the people of Albi concerning the conduct of heresy investigations in their city inspected the jails of the Carcassonne inquisitors, they discovered that many prisoners whose trials were not yet completed were being kept shackled and housed in “cramped and very dark cells.” Some had apparently endured these conditions for five years or more.³⁰

Psychological stress was also consciously applied to those lodged in the inquisitorial prisons. The author of *De inquisitione hereticorum* (composed in the 1260s) had a number of simple but undoubtedly effective suggestions for coercing prisoners. Not surprisingly, a strict regime of close observation and semi-starvation for reluctant witnesses was recommended. In addition, the author suggested that these detainees be allowed contact only with trusted agents who could suggest to them, while feigning pity, that they should confess in order to spare themselves further suffering.³¹

Writing in the late fourteenth century, the Catalan inquisitor Nicholas Eymerich recommended the following methods for dealing with heretics who refused to renounce their errors and seek absolution: they should be kept shackled and well guarded, and visits by anyone other than the prison guardians should be prohibited. At frequent intervals, the inquisitor and the local bishop should summon the prisoners and give them instruction on the nature of correct belief. If these exhortations failed, the inquisitor should choose ten or twelve religious experts, secular and religious clergy as well as lay jurists, to attempt to persuade the prisoners of their errors. Should any remain obdurate despite all these efforts, the inquisitor ought not to be in a rush to turn them over to the secular authorities for execution. (Indeed, Eymerich remarked that, since many heretics demanded execution, regarding it as a glorious form of martyrdom, one should be in no hurry to oblige them.) Instead, these prisoners should be lodged for several months under extremely severe conditions. If this failed to break their will, Eymerich advised that the inquisitor should change tactics and relax the conditions of detention. In addition to promising the prisoners that they would be treated with mercy if they confessed, the inquisitor should see to it that the prisoners’ wives and children tried to persuade them to recant. Only if all these strategies failed should the obdurate heretics be sent to the stake.³²

The inquisitorial manuals’ suggestions for manipulating the conditions of imprisonment so as to elicit confessions seem to have been implemented with

²⁹ Gui, *Manuel*, 1: 182–83.

³⁰ Clément Compayré, *Etudes historiques et documents inédits sur l’Albigeois, le Castrais et l’ancien diocèse de Lavaur* (Albi, 1841), 244. The commissioners ordered that the prisoners should be held under less harsh conditions.

³¹ Antoine Dondaine, “Le Manuel de l’inquisiteur (1230–1330),” *Archivum Fratrum Praedicatorum*, 17 (1947): 105.

³² “Wives” is the term Eymerich specified. Nicholas Eymerich, *Directorium inquisitorum F. Nicolai Eymerici ordinis Praedicatorum, cum commentariis Francisci Pegnae sacrae theologiae ac iuris utriusque doctoris* (Venice, 1595), 514–15. On Eymerich’s career, his manual, and its editorial history, see Agostino Borromeo, “A Proposito de *Directorium inquisitorum* di Nicolás Eymerich e delle sue edizioni cinquecentesche,” *Critica Storica*, 20 (1983): 449–547.

great frequency. For example, in 1263, Raimond Bernard de Flassac, bailiff of the count of Foix, was imprisoned on suspicion of heresy. He confessed, but he did so, he later contended, largely "because of the frightful torment and narrowness of my cell and the hunger I endured almost constantly for a month and two days in that cell."³³

Although the inquisitors made novel uses of imprisonment, they did not create Benthamite panopticons. There was, it seems, little thought given to the rational organization of space within the prisons or the uses to which those spaces might be put. Some prisoners were detained under very severe conditions, shackled and fed only on bread and water.³⁴ Others enjoyed more congenial forms of detention. Indeed, some of the descriptions we have of life in the inquisitorial prisons make them sound a little like the freewheeling jails of eighteenth-century England, in which prisoners were largely allowed to construct their own social world.³⁵

Jean Galand, inquisitor at Carcassonne, felt compelled in 1282 to instruct the guardian of the prison in that city to refrain from exchanging gifts with and making loans to his prisoners and from eating and playing with them.³⁶ Among our best descriptions of life in an inquisitorial prison are those in Jacques Fournier's register. Fournier, bishop of Pamiers, acting in conjunction with the inquisitor of Carcassonne, set up his own inquisitorial tribunal in the early fourteenth century to deal with the vestiges of Catharism in his diocese. One of those who fell afoul of his investigations was Bernard Clergue of Montailou. The records generated in the course of his trial reveal that the Allemans prison in Pamiers was a chaotic place, where Bernard was left free to scheme to secure the release of himself and his brother, who had also been arrested, and to settle scores with his enemies. Bernard was even given a set of keys by one of his keepers so that he could move around at will inside the jail, busily threatening and cajoling his fellow inmates. But even this anarchic prison life could have its uses. To his sorrow, Bernard discovered that he talked too much. It was a simple matter for Fournier to interrogate Bernard's fellow detainees and uncover damaging evidence against him.³⁷ All of this may sound banal; it is not. What the inquisitors had done was to delimit a space in which they could isolate individuals from the outer world and subject them without interruption to an enforced and forcible persuasion. Such a planned and active use of imprisonment for what we

³³ C. Devic and J. Vaissète, *Histoire générale de Languedoc*, ed. Auguste Molinier, 16 vols. (Toulouse, 1872–1904), 8: c. 1481.

³⁴ For a formula to be used in sentencing someone to imprisonment *in muro stricto et in loco artiori in vinculis seu compedibus*, see Gui, *Practica*, 102.

³⁵ On this, see W. J. Sheehan, "Finding Solace in Eighteenth-Century Newgate," in *Crime in England, 1550–1800*, ed. J. S. Cockburn (Princeton, N.J., 1977), 229–45.

³⁶ Collection Doat, vol. 32, fols. 125–26, Bibliothèque Nationale. Printed in Molinier, *L'Inquisition dans le Midi*, 446 n. 1.

³⁷ Jean Duvernoy, ed., *Le Registre d'inquisition de Jacques Fournier (1318–1325)*, 3 vols. (Toulouse, 1965), 2: 277–90. For a similar event, see the testimony of Blanche de Rodès before the inquisitors of Carcassonne in 1308 in which she reported what Raimond Issaurat of Larnat, imprisoned with her at Foix, had told her about heretical activities in Larnat; Annette Pales-Gobilliard, ed., *L'Inquisiteur Geoffroy d'Ablis et les cathares du comté de Foix (1308–1309)* (Paris, 1984), 232–33, 236–37.

would label behavior modification was possibly without parallel in medieval Europe.

THE INQUISITORS WERE ALSO AT THE FOREFRONT in the development of other techniques of rule and coercion. One of the most striking aspects of their work is the effective use they made of documents. Everyone knows that medieval people kept records, but it is not very clear just what use they made of the mounds of parchment they stored away. One of the few scholars to study the use of documents is Michael Clanchy. In his work on record keeping and literacy in medieval England, he has pointed out that the making of documents for immediate administrative use, their preservation as a form of perpetual, unfailing memory, and their use again at a later date for reference are three distinct processes that do not logically follow one from another. Most administrative documents, like tax lists and instructions to officials, were intended to be used only at the time they were fabricated. Once used, they had little further function. Once prepared and stored away, they were seldom used actively or systematically as sources of reference.³⁸ True, records were frequently produced from the archives to be used in litigation or diplomatic negotiations as a way of proving title to a disputed right.³⁹ But they were seldom subjected to an analytic scrutiny designed to produce, not evidence of good title, but information that could be used in either the formulation or the pursuit of policy goals. The analysis of the records of past governmental activity as part of an analytic strategy designed to allow one to rule more effectively, to exert power in a more concentrated and efficient way, seems to have been rare.

The inquisitors used their records in just such an active, analytic fashion. The inquisitorial archives served the immediate purposes for which they were made, that is, to record the trial of arrested heretics, but they were also consciously constructed and used as part of a technology of investigation. The inquisitorial registers were thus active instruments of knowledge *and* coercion. When Bernard de Caux compiled a record of the thousands of interrogations he had conducted in the diocese of Toulouse in the 1240s, he arranged his depositions topographically to facilitate the cross-checking of one deposition with another. Marginal notations were made of inconsistencies and other relevant material.⁴⁰ Through this mechanism, Bernard and his assistants were able to discover that some hundred people had not been completely frank with the Inquisition. These

³⁸ Michael Clanchy, *From Memory to Written Record: England, 1066–1307* (London, 1979), esp. 138–47.

³⁹ See, for example, the massive compilation of documents arising out of the “Great Cause” concerning the succession to the throne of Scotland at the end of the thirteenth century in E. L. G. Stones and Grant G. Simpson, eds., *Edward I and the Throne of Scotland, 1290–1296: An Edition of the Record Sources for the Great Cause*, 2 vols. (Oxford, 1978), esp. 1: 137–62, 2: 296–309.

⁴⁰ It is worth noting that Bernard de Caux’s procedures drew on well-developed aspects of the literate culture of his day. The arrangement of data by place rather than date was a common practice used by archivists to facilitate future reference to their documents. The practice of systematic confrontation of one text with another was, of course, an integral feature of the study of law and theology.

unlucky individuals were re-summoned to explain the discrepancies in their testimony.⁴¹ Similarly, when Bishop Fournier interrogated Guillaume Delaire in February 1325, he had on hand a copy of the deposition that Guillaume had made several years earlier to the inquisitor of Carcassonne. By referring to this old testimony, Fournier was able to catch Delaire out in a patent lie.⁴²

The inquisitors were well aware of the importance of their records and of the care necessary to make them useful. Bernard Gui in his *Practica* noted that it was occasionally not expedient to record all of a suspect's interrogation, since this would clutter the record with too much irrelevant detail. Conversely, he observed that one should not remove too much material from the record of an interrogation, lest it appear truncated. The goal of the record keeping, as Gui saw it, was to produce interrogations that could readily be collated without excessive expenditure of time and labor.⁴³

Writing around 1376, Eymerich offered some useful advice on how an inquisitor might control the information he received. Eymerich had observed that, during the grace period proclaimed by the inquisitors, confessions could be so numerous as to make it impossible for record keepers to follow the normal procedures of the Inquisition. Eymerich advised that the inquisitor prepare a small notebook, one per diocese, in which to note the gist of each confession. In his own hand, Eymerich counseled, the inquisitor should also record the names of those whom the deponents had denounced and who should therefore be summoned for further interrogation.⁴⁴

The archives of the inquisitors could also be put to political uses. In the late thirteenth and early fourteenth centuries, much of Languedoc was disturbed by a major anti-inquisitorial campaign in which the city of Albi figured prominently. The royal vicar of Albi, Guillaume de Pézens, played an important role in this campaign, using the powers of his office to help the members of the anti-inquisitorial party. The inquisitors, however, were able to use information contained in their archives to bring about his downfall. In 1306, Geoffroy d'Ablis, the inquisitor of Carcassonne, searched his tribunal's records and discovered that Guillaume's ancestors had been deeply involved in heresy. His grandmother Alix, a perfected heretic, had been captured at Montségur in 1244 and executed. In the 1250s, other relatives had been condemned to various punishments for their heretical dealings. Since descendants of heretics were barred from public office, Geoffroy d'Ablis was able to secure Guillaume's dismissal from his post.⁴⁵

The records of the Languedocian inquisition were once very extensive. In the early eighteenth century, when the archives of the Carcassonne tribunal were removed to Montpellier and an inventory made, they contained 115 pieces—

⁴¹ Dossat, *Crises*, 243–44.

⁴² Duvernoy, *Le Registre d'inquisition*, 3: 450–51.

⁴³ Gui, *Manuel*, 1: 32–33.

⁴⁴ Eymerich, *Directorium inquisitorum*, 413.

⁴⁵ Yves Dossat, "Le 'Bûcher de Montségur' et les bûchers de l'inquisition," *Cahiers de Fanjeaux*, 6 (1971): 362–65, 374–77. See also Collection Doat, vol. 34, fols. 104^r–108^r; and MS. Lat. 4270, fol. 266^r, Bibliothèque Nationale.

nineteen registers, fifty-six books, thirteen cahiers, nine rolls, and various other items. Forty-three of these documents originated in the thirteenth century while thirty-six came from the first half of the fourteenth, the time when the tribunal was at its most active.⁴⁶

Not surprisingly, the inquisitors were careful of their records. They were kept in chests under lock and key and in locked rooms in locked houses.⁴⁷ The inquisitors were well aware that their records were objects of fear and loathing, perhaps even more so than the inquisitors themselves.⁴⁸ At times, efforts were made to steal and destroy archives. When a number of inquisitors were assassinated in 1242, a brisk trade ensued in the murdered Dominicans' records.⁴⁹ In 1247, one of the Inquisition's clerks and one of its couriers were murdered at Caunes and the registers in their possession burned.⁵⁰

The best-documented effort to steal inquisitorial records took place in the mid-1280s. The inquisitors of Carcassonne had compiled a great quantity of information about the career of the *Bonhomme* Guillaume Pagès, active between 1269 and 1284. From the surviving scraps of this material, it appears that many of the leading citizens of Carcassonne, among them town consuls, royal officials, professors of law, canons of the cathedral of Sainte-Nazaire, the bishop's official, and the archdeacon of Carcassonne, had had intimate dealings with Pagès. By the mid-1280s, these people were beginning to fear for their safety.

To protect themselves, they decided to strike, not at the inquisitors, but at the source of their dangerous knowledge—the registers. One of the Inquisition's agents, a man named Bernard de la Garrigue of Lados, was recruited to steal the registers and burn them. He lacked a major qualification for the job, however; he did not know how to read. He was therefore given as an assistant a professional copier of manuscripts. But, when the two sneaked into the Carcassonne headquarters, they discovered that the inquisitor, Jean Galand, had gone to Toulouse and taken with him the key to the chest in which the registers were kept. Before a second attempt could be mounted against the registers, Galand had gotten wind of the plot and begun to arrest, interrogate, and convict members of the conspiracy.⁵¹

⁴⁶ Dossat, *Crises*, 43–44.

⁴⁷ On inquisitorial records, see Dossat, *Crises*, 30–32.

⁴⁸ Critics not infrequently charged that the inquisitors deliberately falsified and altered their records so as to condemn the innocent. See, on this point, the complaints of the people of Cordes to papal commissioners in 1306 in Compayré, *Etudes historiques*, 244–45. Conversely, during the major anti-inquisitorial agitation against the inquisitors in the early years of the fourteenth century, Bernard Gui charged that the Inquisition's opponents themselves fabricated registers in which the names of innocent people were recorded; Bernard Gui, *De fundatione et prioribus conventuum provinciarum Tolosanae et Provinciae ordinis Praedicatorum*, ed. P. A. Amargier (Monumenta Ordinis Fratrum Praedicatorum, 24, 1961), 201.

⁴⁹ One document was sold for the respectable sum of 40 sous. Dossat, *Crises*, 34.

⁵⁰ Dossat, *Crises*, 169; Devic and Vaissète, *Histoire générale de Languedoc*, 8: cc. 1239–40.

⁵¹ The depositions extracted by the Carcassonne inquisitors concerning this plot are preserved in Collection Doat, vol. 26, fols. 195^v–216^r, 250^r–254^r, 261^v–265^v, 267^v–271^v. Excerpts have been published by M. Mahul, ed., *Cartulaire et archives des communs de l'ancien diocèse et de l'arrondissement administratif de Carcassonne*, 6 vols. (Paris, 1857–82), 5: 638–43. The plot is discussed in J.-M. Vidal, *Un inquisiteur jugé par ses "victimes": Jean Galand et les Carcassonnais (1285–1286)* (Paris, 1903); and in Michèle Lebois, "Le Complot des Carcassonnais contre l'inquisition (1283–1285)," in *Carcassonne et sa région* (Actes des XLI^e et XXIV^e Congrès d'Etudes Régionales Tenus par la Fédération Historique

In addition to compiling and safeguarding their own records, the inquisitors endeavored to collect works written by heretics. Bernard Gui, for example, included in his *Practica* a copy of a short treatise on the death of Pierre-Jean Olivi that the heretics known as Beguins were accustomed to read aloud during their meetings.⁵²

Some inquisitors subjected the records their institution had amassed to a form of scientific scrutiny. Like modern historians, they were interested in discovering what heretics believed and what practices they followed. Through their researches, the inquisitors hoped to be able to identify and interrogate heretics more effectively. Over the years, they produced a series of manuals designed to convey necessary information concerning not only the authority and procedures of the Inquisition but also the nature of different heresies, together with suggestions on how most effectively to prosecute different types of heretics. The earliest of these, the *Processus inquisitionis* (possibly composed in 1248 or 1249), was a very simple document, being little more than a legal formulary.⁵³ But these manuals quickly grew in size, complexity, and sophistication.

The manuals contained a great diversity of material. Some were mere formularies, with examples of the standard legal documents that inquisitors prepared during their investigations. Some were treatises on the beliefs and practices of the different heretical sects. And some contained practical advice on how to interrogate suspects. The treatise known as *De inquisitione hereticorum*, for example, suggests that reluctant witnesses might be persuaded to confess by threatening them with death or telling them that other witnesses had already implicated them.⁵⁴ The best known of these manuals is the *Practica* of Bernard Gui, written around 1323. It is in part a formulary, containing examples of 170 different documents. It also contains an extensive discussion of the powers of the inquisitors. A large part of the *Practica* is devoted to a treatise on various heretical sects, discussing their origins, beliefs, and practices. Finally, Bernard, noting that different types of heresy require different modes of interrogation, included suggestions on the best strategies to pursue in questioning followers of particular heresies, complete with some sample dialogues.⁵⁵

In addition to producing such treatises, inquisitors apparently collected each other's works. During the seventeenth century, documents originally forming

du Languedoc Méditerranéen et du Roussillon et par la Fédération des Sociétés Académiques et Savantes de Languedoc-Pyrénées-Gascogne, Carcassonne, 17–19 mai 1968), 159–63.

⁵² Gui, *Manuel*, 1: 190–93. The Catalan inquisitor, Nicholas Eymerich, writing ca. 1376, makes it clear that he had studied a number of treatises on magic seized from necromancers who had appeared before his tribunal; Eymerich, *Directorium inquisitorum*, 338.

⁵³ Yves Dossat, "Le Plus ancien manuel de l'inquisition méridionale: Le *Processus inquisitionis* (1248–49)," *Bulletin Philologique et Historique (jusqu'à 1715) du Comité des Travaux Historiques et Scientifiques* (1948–50): 34, 37. This treatise has been edited by Ad. Tardif, "Document pour l'histoire du *Processus per inquisitionem* et de l'*Inquisitio heretice pravitatis*," *Nouvelle Revue Historique de Droit Français et Etranger*, 7 (1883): 670–78. An English translation can be found in Walter L. Wakefield, *Heresy, Crusade and Inquisition in Southern France, 1100–1250* (Berkeley, Calif., 1974), 250–58.

⁵⁴ Dondaine, "Le Manuel de l'inquisiteur," 105.

⁵⁵ Gui, *Manuel*, 1: 6–9. See, for example, the discussion of how to interrogate Waldensian heretics in *Manuel*, 1: 64–83. See also Nicholas Eymerich's advice to inquisitors on how to conduct interrogations in *Directorium inquisitorum*, 425–35.

part of the archives of the inquisition of Carcassonne were copied (ultimately preserved in the Bibliothèque Nationale), so we now know that the Carcassonne inquisitors possessed copies of Bernard Gui's *Practica*, Rainier Sacconi's *Summa de Catharis et Leonistis* (composed around 1250), the *Disputatio inter catholicum et Paterinum haereticum* (possibly written by Gregory of Florence, bishop of Fano, who died around 1240), and the section of Stephen of Bourbon's *De septem donis Spiritus Sancti* dealing with the Waldensians.⁵⁶

MUCH OF THE ABOVE COULD LEAD ONE to the conclusion that the inquisitors were possessed of superior techniques for discovering the truth. Their refinement of methods of interrogation and coercion, their familiarity with the nature of the people and the deviancy with which they were dealing, and their skillful and active use of record keeping made it possible, when necessary, to discover facts that their victims would have preferred to see kept obscure. The success the inquisitors enjoyed in ferreting out heretics is eloquent testimony to just how effective these medieval techniques could be when wielded with enthusiasm and determination. The inquisitors were not, however, limited to the facts. Their investigatory techniques allowed them to create their own, tailor-made realities. Through their interrogation procedures, the inquisitors could make manifest the ideas, fears, and fantasies that had previously resided only in their own minds. In a sense, they could make these phantasms objectively real.

For example, one of the individuals Jacques Fournier interrogated was the commander of the leprosarium in Pamiers, Guillaume Agasse.⁵⁷ Agasse's confession is part of a dark episode of early fourteenth-century history in which King Philip V launched a major persecution of lepers for allegedly planning to poison wells and streams all over France.⁵⁸ Agasse's arrest and prosecution grew out of this campaign. Under torture, Agasse produced an amazing tale of how he and delegates from forty other leprosaria in the Midi had held a great council in Toulouse in May 1320. There they had agreed to poison all the healthy people in their corner of France so that they would either die or become lepers. For his part in this utterly fantastic and nonexistent conspiracy, Agasse was sentenced to imprisonment on July 5, 1322. As far as we know, he died in Fournier's prison.

The most famous example of inquisitorial ability to bend truth to its needs was the trial and destruction of the order of the Knights Templar, in which King Philip IV of France made good use of inquisitorial techniques and personnel. When the French Templars were arrested en masse in 1305, they were first interrogated by royal agents, who made liberal use of torture. When the

⁵⁶ Dossat, *Crises*, 50–51; Molinier, *L'Inquisition dans le Midi*, 35–39.

⁵⁷ Agasse's deposition is recorded in Duvernoy, *Le Registre d'inquisition*, 2: 135–47.

⁵⁸ On this episode, one can consult H. Chrétien, *Le Prétendu complot des Juifs et lépreux en 1321* (Chateauroux, 1887); J.-M. Vidal, "La Poursuite des lépreux en 1321 d'après des documents nouveaux," *Annales de Saint-Louis-des-Français*, 4 (1900): 419–78; Vincent Raymond Rivière-Chalan, *La Marque infâme des lépreux et des chrétiens sous l'Ancien Régime: Des Cours des miracles aux cagoterres* (Paris, 1978), 39–63; and Malcolm Barber, "Lepers, Jews and Moslems: The Plot to Overthrow Christendom in 1321," *History*, 66 (1981): 1–17.

Templars had had a chance to learn their lines, they were turned over to ecclesiastical inquisitors under the direct supervision of the general inquisitor of France, who happened to be the king's Dominican confessor, Guillaume Imbert. The Templars confessed to an array of monstrous untruths—denial of the church and its sacraments, homosexual practices, demon worship, and so forth. Some Templars tried to resist when it appeared that papal commissioners delegated to investigate the affair would listen to them. But, when these Templars began to recant their confessions, they were simply treated as relapsed heretics and burned. The other Templars decided that it would be best to stick to their stories. As a result, the entire order was suppressed.⁵⁹

Although no one doubts the capacity of people in any era to lie and deceive, the medieval inquisitors had perfected techniques by which the very fabric of reality could be altered. By the mid-thirteenth century, the creation of various fantasies and their projection onto certain outgroups, such as the widespread belief that Jews indulged in ritual murder, had become an integral feature of West European culture.⁶⁰ The inquisitors had devised methods by which power and coercion could be used to give such fantasies a legally validated and socially accepted reality. Not only could despised and rejected members of society be made the objects for the projection of the fears and fantasies of Western culture but they, or virtually anyone else, could also be made to admit that those fantasies were true and to suffer the terrible consequences of being guilty of behavior that existed only in the imaginings of their persecutors. The inquisitors could, if they wished, script roles for almost anyone who appeared before them and make them play these pre-assigned and largely pre-written parts. As the Franciscan Bernard Délicieux told Philip IV, had St. Peter and St. Paul appeared before the inquisitors, they would have been found to be heretics.⁶¹

NOT ONLY DID THE INQUISITORS CREATE NEW REALITIES, they also created new social groupings through the elaboration of a political economy of punishment. Medieval Europe knew many repressive institutions, but few of them planned

⁵⁹ Good, recent discussion of this affair can be found in Malcolm Barber, *The Trial of the Templars* (Cambridge, 1978); and Peter Partner, *The Murdered Magicians: The Templars and Their Myth* (New York, 1982), 59–85.

⁶⁰ See G. I. Langmuir, "Thomas of Monmouth: Detector of Ritual Murder," *Speculum*, 59 (1984): 820–46; "The Knight's Tale of Young Hugh of Lincoln," *Speculum*, 47 (1972): 459–82; and R. I. Moore, *The Formation of a Persecuting Society: Power and Deviance in Western Europe, 950–1250* (Oxford, 1987), esp. 116–23.

⁶¹ MS. Lat. 4270, fol. 139^{r-v}. (This manuscript is a seventeenth-century copy, made on the order of Etienne Baluze, of the records of the 1319 heresy trial of Bernard Délicieux. See Molinier, *L'Inquisition dans le Midi*, 29–30.) That denunciation to the inquisitors was tantamount to conviction seems to have been apparent to the people of Languedoc. Indeed, if we can believe the charges made before the archbishop of Narbonne by Jean Martin, himself a Dominican, sometime in the 1270s or 1280s, this knowledge was put to good use by the inquisitors. According to Friar Martin, the inquisitors proceeded against certain suspects by informing their wives that their husbands had already been denounced, and that the only way they could save themselves would be to appear before the inquisitors and confirm their guilt. Driven by the entreaties of their wives, the men would do so and attest to the truth of whatever charges were lodged against them; MS. Lat. 4270, fol. 126^{r-v}.

social uses for the penalties they inflicted. If one committed a crime and was convicted, one received the specified penalty, whether a fine or one of a number of various afflictive punishments—whipping, mutilation, execution. Little attention was given to how punishments might be used to modify the behavior of the person who had been convicted or that of others. The inquisitors, however, conceived of their activity not so much as punitive but as corrective, an effort to reconcile sinners with the church. The penalties they handed out were officially interpreted as penances for sin. Also, the inquisitors wanted information about other heretics from those who appeared before them. By developing a flexible and varied scheme of punishment, the inquisitors were better able to achieve these twin goals.

The penalties they could inflict ran the gamut from the relatively light to the definitely harsh. Among the lighter penances at the inquisitors' command were monetary fines and requirements that penitent heretics underwrite various pious causes. More severe were those penances involving public infamy (*penitentiae confusae et confusibiles*). These often required penitents to undertake pilgrimages, some of which could last for years. The most severe penalties included imprisonment (often perpetual) and confiscation of property. Those who refused to recant their heretical beliefs or who, having recanted, later returned to their errors were remanded to the secular arm of the state and invariably burned at the stake.⁶²

The efforts to construct a political economy of punishment may explain why the medieval Inquisition was not especially bloody. Bernard Gui, active as inquisitor of Toulouse in the early fourteenth century, imposed 930 sentences on convicted heretics during his career but sent only forty-two to the stake. Bernard imprisoned 307 people for various terms, sentenced 143 to wear crosses, and dispatched nine on pilgrimages. He also ordered the exhumation and cremation of the bodies of sixty-nine deceased heretics and the destruction of twenty-two houses.⁶³

The inquisitors' flexible system of punishments allowed them to fashion a readily identifiable and manipulable deviant subgroup. Penitent heretics were not reintegrated into the society of the faithful but were carefully marked out and set apart from everyone else, a separation made manifest by the physical symbols that most had to wear. Almost all convicted heretics not kept in prison were required to wear yellow crosses sewn on their clothing, the arms of the crosses two-and-a-half fingers in breadth, two-and-a-half palms in height, and two palms in width. One cross was to be worn on the chest and one on the back. In addition, false witnesses had to wear red tongues.⁶⁴ These symbols were not to be laid aside, indoors or out, or obscured by other clothing. If, during their trials, suspects committed perjury, a second transverse arm was added to the cross. Penitents were also required to present themselves at Mass at their parish

⁶² See the discussions in Molinier, *L'Inquisition dans le Midi*, 397–451; and Maisonneuve, *Etudes sur les origines de l'inquisition*, 301–06.

⁶³ Douais, *Documents pour servir*, 1: ccv.

⁶⁴ Lea, *History of the Inquisition*, 1: 468. See also the discussion of the imposition of crosses and other symbols as a punishment in Tanon, *Histoire des tribunaux*, 494–501.

church every Sunday and every feast day dressed in a minimum of clothing and carrying rods with which the priest beat them before the assembled congregation.⁶⁵ Penitents were also obliged to attend all solemn church processions in similar garb and to be scourged at their conclusion.

Convicted heretics almost never completed their term of penance. Even if released from prison and allowed to set aside their crosses, they remained ex-heretics. The inquisitors expressly reserved the right to impose new penances or reimpose old ones.⁶⁶ Penitent heretics thus entered a special, clearly defined marginal social status. Marked out from their neighbors by readily apparent physical signs and subjected to public and degrading rituals, they could, at any time, have inflicted on them new and heavier forms of punishment. The descendants of heretics also suffered disabilities, being disinherited and debarred from public office.

The forced adoption of an inferior, degraded social status was a fate not limited to ex-heretics in medieval Europe. Jews and lepers were undergoing similar experiences in the twelfth and thirteenth centuries.⁶⁷ But the inquisitors not only created a stigmatized social group but often successfully enlisted its members in the task of repressing those heretics who remained at large.

It was not uncommon for ex-heretics to enter the employ of the inquisitors.⁶⁸ Raimond Gros, a Cathar *Bonhomme* whose conversion to orthodoxy in 1236 did much to facilitate the work of the inquisitors of Toulouse, spent the rest of his days in the Dominican convent in that city.⁶⁹ Another member of this deviant underworld was Bernard de la Garrigue. Bernard had been a devout Cathar, even receiving the *consolamentum* and becoming a *Bonhomme*. He had been active in the sect when it still had a fairly elaborate organization, with dioceses paralleling those of the Roman Catholic Church, each ruled by a bishop assisted by officers called younger and elder sons. He himself had been the elder son of the diocese of Albi. When he fell into the hands of the inquisitors, Garrigue, unlike most *Bonshommes*, did not go to the stake but instead confessed and sought absolution. He became a minor functionary for the Carcassonne inquisitors. Similarly, Raimond Peyre, a less prominent and devoted heretic from the county of Foix, who merely favored the Cathars, had spent some time in the early

⁶⁵ See the formula in Gui, *Practica*, 37–39. See also the discussion in Tanon, *Histoire des tribunaux*, 510–13.

⁶⁶ This was an almost invariable element in the sentences imposed by Bernard Gui. See his “*Liber sententiarum inquisitionis Tholosane*,” in Limborch, *Historia inquisitionis*, 1, 9, 13, 32, 40, 45, 78, 97, 117, 158, 177, 182, 185, 191, 202, 213, 215, 218, 229, 244, 267–68, 295–96, 298, 332, 338, 341, 347, 364. See also Tanon, *Histoire des tribunaux*, 432–33.

⁶⁷ On the Jews, see G. I. Langmuir, “Anti-Judaism as the Necessary Preparation for Anti-Semitism,” *Viator*, 2 (1971): 386–87; Salo W. Baron, *A Social and Religious History of the Jews*, vol. 11, 2d edn. (New York, 1967), 3–13, 96–106; on the lepers, see Moore, *Formation of a Persecuting Society*, 45–60.

⁶⁸ An inquisitorial account from 1255 reveals that at least two former heretics were serving as *nuncii* for the inquisitors of Toulouse; Cabié, “*Comptes des inquisiteurs des diocèses de Toulouse*,” 131. Four other “converted” heretics were also being supported by the inquisitors; *ibid.*, 133.

⁶⁹ Guillaume Pelhisson, *Chronique de Guillaume Pelhisson*, ed. and trans. Jean Duvernoy (Toulouse, 1958), 40. I have been unable to consult the Latin edition of this treatise edited by C. Douais in *Les Sources de l'histoire de l'Inquisition au moyen âge* (Paris, 1881). There is an English translation of Pelhisson's chronicle in Wakefield, *Heresy, Crusade and Inquisition*, 207–36.

fourteenth century lodged in the prison (generally known as the *mur*) at Carcassonne. After his release, he occasionally worked for the inquisitors, carrying letters of citation to suspects and witnesses in the county of Foix.⁷⁰

The inquisitors used ex-heretics as minor servants, and they also tried to make of them a tool with which to catch other heretics. Penitent heretics, as a condition of their absolution and reconciliation with the church, were required to promise to assist in the persecution of heresy. The inquisitors saw to it that this obligation was taken seriously. For example, in 1246, Bernard de Caux and Jean de Saint-Pierre sentenced Estolt de Roqueville of Montgiscard and Arnaud-Pons Saquet of Lanta to perpetual imprisonment for failing to arrest heretics known to them. Guillem Bérenger of Arzens, who had been allowed to lay aside the crosses he had been sentenced to wear, was in 1254 ordered to take them up again by the bishop of Carcassonne (who had established his own inquisitorial tribunal) for failing to secure the arrest of a heretic he had chanced upon in the town of Limoux.⁷¹

In return for a relaxation of their penalties, some ex-heretics were willing to contract with the inquisitors to arrange the capture of their fellow sectaries. This was a sufficiently common occurrence for Bernard Gui to include in his *Practica* a formula for absolving the punishment of an ex-heretic who had procured the arrest of a heretic.⁷² In the mid-thirteenth century, Amblard Vassal was given license by agents of the inquisitors to consort with heretics and lure them to places where they could be captured.⁷³ In 1305, Guillaume Pierre, a Cathar believer who had abandoned his heretical beliefs, secured the arrest of the *Bonshommes* Jacques Autier and Prades Tavernier by luring them to Limoux under the pretext of administering the *consolamentum* to a dying woman.⁷⁴

One of the more dramatic examples of the use the inquisitors could make of such people is the career of Arnaud Sicre. Arnaud was one of the children of Sibille den Balle, a fervent Cathar from the county of Foix. In the early fourteenth century, she had been convicted of heresy and burned at the stake; all her property had been confiscated and her children thus disinherited. Arnaud decided that he might be able to recover his inheritance if he could secure the arrest of a *Bonhomme*. A search south of the Pyrenees resulted in his discovery of a heretic named Guillaume Belibaste in the Aragonese town of San Mateo. Belibaste had escaped from the *mur* in Carcassonne and fled across the Pyrenees for safety. Arnaud promptly brought this information to the attention of Bishop Fournier in Pamiers. With Fournier's permission, Arnaud masqueraded as a heretical sympathizer and managed to penetrate the group of Cathar

⁷⁰ Duvernoy, *Le Registre d'inquisition*, 3: 426–27. For more information on Peyre, see my "Factional Politics in a Medieval Society: A Case Study from Fourteenth-Century Foix," *Journal of Medieval History*, 14 (1988): 233–50.

⁷¹ Molinier, *L'Inquisition dans le Midi*, 320–21.

⁷² Gui, *Practica*, 48–49.

⁷³ Annie Cazenave, "L'Entraide cathare et la chasse à l'hérétique en Languedoc au XIII^e siècle," *Actes du 96^e Congrès National des Sociétés Savantes (Toulouse, 1971)*, Section de Philologie et d'Histoire jusqu'à 1610: Volume 2, *Pays de Langue d'Oc, Histoire et Dialectologie*, 122 n. 53.

⁷⁴ Guillaume remained in the service of the inquisitors and took part in the mass arrest of the people of Montailhou in 1308. Duvernoy, *Le Registre d'inquisition*, 2: 57–58.

exiles from Foix who had taken up residence beyond the Pyrenees. Eventually, Arnaud managed to lure Belibaste and some of his followers into the diocese of Urgel, where he contrived their arrest.⁷⁵

EVEN THOUGH I HAVE EMPHASIZED the effectiveness of the political tools wielded by the inquisitors, I do not wish to exaggerate their success or suggest that the exercise of power is a matter of pure technology. A consideration solely of the application of techniques of governance gives only a partial account of the acquisition and utilization of power. Governing techniques are deployed in the context of specific social relations, and the forces present in each constellation of social relations play a large role in determining how effective or ineffective they will be. Some aspects of medieval Languedocian society put restraints on how effectively the inquisitors could utilize their often ingenious techniques.

In carrying out the duties of their office and in dealing with the opposition they encountered, the inquisitors must have been hampered, like all other medieval administrators, by the fact that they were not the agents of a unified political organization endowed with a legally recognized monopoly of authority. The inquisitors operated in a complex and fragmented political world and were often confronted with the necessity of negotiating arrangements with other power holders. On occasion, the agents of secular lords attempted to infringe on their jurisdiction, like the servants of Alfonse de Poitiers in the county of Toulouse who in the 1250s were burning relapsed heretics sentenced by the inquisitors only to terms of imprisonment, or the secular court in Brissac that in the 1330s presumed to condemn three people for sorcery.⁷⁶

A more important problem for the inquisitors than this squabbling over jurisdiction, an endemic feature of the political life of medieval Europe, was their need for the active assistance of local lords in order to carry out their tasks. On the one hand, inquisitors were dependent for their salaries and other financial assistance on secular authorities, who were required to pay them out of the profits derived from property confiscated from condemned heretics.⁷⁷ Even the prison used by the Carcassonne inquisitors was built at royal expense and the prisoners lodged there supported out of the royal coffers.⁷⁸ On the other hand, despite the fact that they employed some agents of their own, the inquisitors were largely reliant on royal or seigneurial officials for the actual business of hunting down and arresting fugitive heretics. Although the inquisitors had the right to excommunicate anyone who refused to assist them, they often found secular authorities, perturbed by local hostility to the inquisitors, less than enthusiastic partners.

The Capetian kings generally supported the inquisitors' work, but there were

⁷⁵ Arnaud Sicre's deposition is printed in Duvernoy, *Le Registre d'inquisition*, 2: 20–81.

⁷⁶ For Alfonse de Poitiers, see Devic and Vaissète, *Histoire générale de Languedoc*, 8: cc. 1409–10; for the court in Brissac, J.-M. Vidal, ed., *Bullaire de l'inquisition française au XIV^e siècle et jusqu'à la fin du Grand Schisme* (Paris, 1913), 260–61.

⁷⁷ Vidal, *Bullaire de l'inquisition française*, xxxix.

⁷⁸ Devic and Vaissète, *Histoire générale de Languedoc*, 8: c. 1206.

times when the royal government adopted an ambivalent, if not hostile, attitude toward cooperation with them.⁷⁹ Lesser royal agents, if so disposed, were in a favorable position to thwart the inquisitors. Until Geoffroy d'Ablis made the fortunate discovery that Guillaume de Pézens was descended from heretics and thus ineligible for public office, Guillaume had exploited his post as royal vicar of Albi to further the work of the anti-inquisitorial league active in the early years of the fourteenth century. Among other things, he used his authority to force unwilling townspeople to contribute to the subsidies raised to support the league's work.⁸⁰ During the same period, his lieutenant, Pierre Nicholay, acted to block, in at least part of the diocese of Albi, the publication of the inquisitor of Carcassonne's excommunication of the royal investigator Jean de Picquigny, who had forcibly removed prisoners from the inquisitors' prison and lodged them in the royal jails.⁸¹

Even the king himself was at times willing to interfere with the Inquisition. Philip IV, confronted with widespread anti-inquisitorial agitation in Languedoc and involved in disputes with the papacy, for several years lent some assistance to the inquisitors' opponents. Moved by their complaints, he instructed his seneschal of Carcassonne in the early 1290s to assist the inquisitors in arresting suspected heretics only following consultation with the judges of the royal court and only when convinced, either by a suspect's confession, public report, or the testimony of credible witnesses, that the person in question was indeed a heretic.⁸²

The papacy, which had created and fostered the institution, was itself at times willing to trim its powers. The inquisitors found that popes were often willing to order investigations into their conduct, to lighten or restrict the penalties they could impose, to instruct them not to molest certain groups of individuals, to insist, as did Pope Clement V, that they act only in conjunction with the local bishop, or even to suspend inquisitorial activity altogether.⁸³

Not only did the inquisitors have to operate in a context of widely scattered political authority and the reluctance of other power holders to aid them, but they also had to deal with a population that was anything but an inert and pliant mass waiting passively to be shaped by their techniques. The acquisition of power is not a zero-sum game, in which any accretion of resources to one player means a necessary and equal decrease in the resources of all other players. The

⁷⁹ On this issue, see Alan Friedlander, "Les Agents du roi face aux crises de l'hérésie en Languedoc, vers 1250–vers 1350," *Cahiers de Fanjeaux*, 20 (1985): 199–220.

⁸⁰ MS. Lat. 4270, fol. 266^v.

⁸¹ B. Hauréau, *Bernard Délicieux et l'inquisition albigeoise (1300–1320)* (Paris, 1877), 176–87.

⁸² Devic and Vaissète, *Histoire générale de Languedoc*, 8: cc. 273–76. These orders were rescinded in 1298; *ibid.*, 8: cc. 276–78.

⁸³ On investigating the inquisitors, see Vidal, *Bullaire de l'inquisition française*, 17–19; on restricting penalties, see Tanon, *Histoire des tribunaux*, 546–47; and Dossat, *Crises*, 171; on groups under the pope's protection, see Pope John XXII on the people of Montpellier; Vidal, *Bullaire de l'inquisition française*, 44, 349–50. Pope Clement V's order was decreed at the Council of Vienne in 1312. See the discussion in Tanon, *Histoire des tribunaux*, 344–46. For the inquisitors' complaints about these measures, see Collection Doat, vol. 30, fols. 90^r and following; and Devic and Vaissète, *Histoire générale de Languedoc*, 9: 334–37. See also Vidal, *Bullaire de l'inquisition française*, 9–11. Pope Gregory IX suspended the Dominican inquisition in Toulouse in 1238.

inquisitors may have created an unusually "powerful" organization, but they had not thereby reduced the fund of power available to everyone else in Languedoc. When the inquisitors aroused opposition, as they frequently did, they found that their opponents could be very formidable.

In the 1230s and 1240s, an armed opposition by professed Cathars and their supporters resulted in a massacre in 1242 of a number of inquisitors at Avignonet by members of the heretical garrison of the castle of Montségur. A major military effort involving a siege of almost a year was subsequently required to reduce this fortress. The Inquisition also often provoked riots and boycotts. In the first two decades of inquisitorial activity, outbreaks occurred in Toulouse, Albi, and Narbonne.⁸⁴ At the end of the thirteenth century and in the early years of the fourteenth, a major campaign against the Inquisition took shape in the cities of Albi and Carcassonne. Along with riots and boycotts of the Dominican order, a veritable anti-inquisitorial league was created that petitioned the French king and the pope and persuaded some public authorities to raise funds to support its activities.⁸⁵

It is not easy to assess the degree of success of such opposition, but it would be a mistake to regard it all as fruitless. The people of Albi, for example, although they did not succeed in winning the release of the people whom their bishop, Bernard de Castanet, had condemned for heresy in 1299 and 1300, nevertheless managed to secure a papal investigation not only of the bishop's inquisitorial proceedings but also of his general conduct in office.⁸⁶ Bishop Castanet was eventually cleared of all wrongdoing by the papacy, but he was transferred to another see, and for a time the temporalities of his diocese were seized by the royal government.

THE INQUISITORS OF LANGUEDOC developed some very successful techniques with which they could manipulate social relations within their environment. Even though little that they did can be regarded as completely novel, since most of the methods they used were being employed in one fashion or another by contemporary political leaders, their development of these techniques, and combination of them into a coordinated whole, was unique. Their systematic use of imprisonment allowed them to extract men and women from the supporting context of their ordinary web of social relationships and subject them to a prolonged and

⁸⁴ See Emery, *Heresy and Inquisition in Narbonne*; Dossat, *Crises*; and Pelhissou, *Chronique*, 22–24, 30–38, 44–47.

⁸⁵ This anti-inquisitorial agitation can be studied in Michel de Dmitrewski, "Fr. Bernard Délicieux, O.F.M., sa lutte contre l'inquisition de Carcassonne et d'Albi, son procès, 1297–1319," *Archivum Franciscanum Historicum*, 17 (1924): 183–218, 313–37, 457–88; 18 (1925): 3–32; and Hauréau, *Bernard Délicieux*.

⁸⁶ The records of those condemned by the bishop have been published by Georgene W. Davis, ed., *The Inquisition at Albi, 1299–1300: Text of Register and Analysis* (New York, 1948). For the political setting, see Jean-Louis Biget, "Un Procès d'inquisition à Albi en 1300," *Cahiers de Fanjeaux*, 6 (1971): 273–341. Documents relevant to the papal investigation are published in Douais, *Documents pour servir*, 2: 302–49. The record of the investigation into the bishop's general conduct is preserved in Collectorie 404, Archivio Segreto Vaticano.

forcible period of persuasion. Their active use of record keeping allowed them to construct an analytic grid that was capable of generating much useful information, once a suspected heretic had been placed within it. Finally, their construction of a political economy of punishment enabled them to insert into a degraded and permanently marginalized subgroup of penitents the people who passed through their hands. Not only did this group serve to witness to the dreadful consequences of heretical belief, its members could also sometimes be used in the campaign against those heretics who remained at large. The work of the inquisitors of Languedoc is persuasive testimony to the power medieval rulers could generate through the careful and determined application of these techniques.

Yet the degree of success enjoyed by the inquisitors should not be exaggerated. Their deployment of the investigative and manipulative techniques they had developed was limited by their political context. And it should be remembered that their techniques were part of a repressive agenda whose nature was essentially negative. Inquisitorial methods were designed not to activate the resources of the inquisitors' society but to repress dangerous ideological tendencies that had appeared within it. Many of the inquisitors' techniques depended for their effectiveness on a strategy of isolation, whether that isolation took the form of committing a suspect to prison for extended interrogation or of giving a confessed and penitent heretic a disadvantaged socio-juridical status. These methods, effective as they were, were primarily intended to stigmatize the people to whom they were applied.

The inquisitors' techniques for seeking out, isolating, breaking down, and condemning heretics or imagined heretics were not effective in generating active support for their work. Inquisitors did manage to enlist in their service many ex-heretics but only by placing them in a degraded subgroup. The techniques devised or perfected by the inquisitors were not capable of activating any significant segments of their society. I am tempted to suggest that this same limitation might well be typical of the other governing institutions that were taking shape in this period. One could argue that the rulers of Western Europe in the twelfth and thirteenth centuries were fashioning techniques that allowed them to organize more effectively their own efforts and to repress with greater success undesirable behavior by their subjects, but that they had not yet discovered how to mobilize on a large scale the resources of the societies that they ruled. But this is a question that can only be resolved through further investigation of the application of political power during the Middle Ages.

Monumental Building as an Indicator of Economic Trends in Northern Rus' in the Late Kievan and Mongol Periods, 1138–1462

DAVID B. MILLER

IN RECENT YEARS, historians have employed quantitative methods to address problems of the socioeconomic, political, and cultural history of medieval Europe in new ways or with a sophistication heretofore impossible. Although evidence for many areas and for the period before the Black Death (1340s) is sketchy and difficult to interpret, we now have many socioeconomic profiles of towns and regions. Statistical methods have also enriched broader studies of demography, urban networks, and long-term economic trends.¹

The same cannot be said about our knowledge and understanding of economic trends in Kievan and Mongol Rus', and for good reason. The earliest documentary evidence capable of being analyzed statistically relates to the last third of the fifteenth century and mostly to Novgorod.² Historians, therefore, have relied heavily on narrative sources. As a result, their conclusions about economic trends in Kievan Rus', the economic impact of the Mongol invasion

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¹ On comparative demography and town formation, see J. C. Russell, *The Control of Late Ancient and Medieval Population* (Philadelphia, Pa., 1985); J. C. Russell, *Medieval Regions and Their Cities* (Bloomington, Ind., 1972); Ester Boserup, *Population and Technological Change* (Chicago, 1981); and works of Gilbert Rozman, particularly *Urban Networks in Russia, 1750–1800, and Premodern Periodization* (Princeton, N.J., 1976), 3–85. On agricultural trends, see Wilhelm Abel, *Agricultural Fluctuations in Europe* (New York, 1980); Georges Duby, *Rural Economy and Country Life in the Medieval West* (Columbia, S.C., 1968); M. M. Postan, *The Medieval Economy and Society: An Economic History of Britain, 1100–1500* (Berkeley, Calif., 1972), esp. 27–31, on pitfalls of interpreting pre-plague statistics; Norman Pounds, *An Economic History of Medieval Europe* (London, 1974); Harry A. Miskimin, *The Economy of Early Renaissance Europe, 1300–1460* (Englewood Cliffs, N.J., 1969); Peter Clark, ed., *Country Towns in Pre-Industrial England* (New York, 1981); and local studies in n. 5, below. M. T. Clanchy, *From Memory to Written Record: England, 1066–1307* (Cambridge, Mass., 1977); and Simon Franklin, "Literacy and Documentation in Early Medieval Russia," *Speculum*, 60 (1985): 1–38, are examples of statistical methods in studying medieval culture.

² Brilliantly worked by A. L. Shapiro, *et al.*, *Agrarnaia istoriia severo-zapada Rossii, vtoraiia polovina XV–nachalo XVI v.* (Leningrad, 1971); and Shapiro, *Problemy sotsial'no-ekonomicheskoi istorii Rusi XIV–XVI vv.* (Leningrad, 1977). Richard Hellie, *Slavery in Russia, 1450–1725* (Chicago, 1982), begins where our period ends.

and tribute, causes and characteristics of economic recovery, regional trends, the distribution of wealth, and the relationship of economic change to politics and culture have been qualified, tentative, or tendentious.³ It is noteworthy that Charles Halperin's 1985 book and the 1982 study by V. T. Pashuto and others, one a Western and the other a Soviet overview of the period, while based on the most up-to-date literature, say little about economic matters. I would argue that a study of the records of monumental building, meaning masonry or brick construction, can provide a rough yardstick of economic trends, and that their information about patronage can illuminate relationships between economic life and matters of culture and politics.⁴ A digression concerning the economic and social history of preindustrial Europe and other civilizations can help us to understand how such a study might proceed.

Medievalists have commonly accepted that the construction of cathedrals, guild halls, and monasteries, and the building and extension of town walls, were evidence of a long-term expansion of the European economy from the tenth through the thirteenth centuries. Historians have also used construction and patronage records to compare the economic vitality of regions and the distribution of wealth among social classes. In the history of China, Japan, and other ancient civilizations, construction projects—including some of unparalleled magnitude—correlate well with periods of economic growth (but not always well-being), even if patterns of patronage and methods and economics of building varied greatly. Ancient Middle Easterners, Indo-Europeans, Mesoamer-

³ The list is impressive: S. B. Veselovskii, *Feodal'noe zemlevladienie v severo-vostochnoi Rusi* (Moscow, 1947); B. A. Rybakov, *Remeslo drevnei Rusi* (Moscow, 1948), 539–776; A. M. Sakharov, *Goroda severo-vostochnoi Rusi XIV–XV vekov* (Moscow, 1959); N. N. Voronin, *Zodchestvo severo-vostochnoi Rusi*, 2 vols. (Moscow, 1961–62); Jerome Blum, *Lord and Peasant in Russia from the Ninth to the Nineteenth Century* (Princeton, N.J., 1971), 13–23, 57–69, 117–34; Bertold Spuler, *Die Goldene Horde: Die Mongolen in Russland, 1223–1502* (Wiesbaden, 1965), 387–452; L. V. Cherepnin, *Obrazovanie russkogo tsentralizovannogo gosudarstva v XIV–XV vekakh* (Moscow, 1960), 149–452; V. V. Kargalov, *Feodal'naia Rus' i kochevniki* (Moscow, 1967), 190–217; Carsten Goehrke, *Die Wüstungen in der Moskauer Rus': Studien zur Siedlungs-, Bevölkerungs- und Sozialgeschichte* (Wiesbaden, 1968); Michel Roublev, "The Periodicity of the Mongol Tribute as Paid by the Russian Princes during the Fourteenth and Fifteenth Centuries," *Forschungen zur osteuropäischen Geschichte*, 15 (1970): 7–13; Michel Roublev, "Le tribut aux Mongoles d'après les Testaments et Accords des Princes russes," *Cahiers du monde russe et soviétique*, 7 (1966): 487–530; Janet Martin, "The Land of Darkness and the Golden Horde: The Fur Trade under the Mongols, XIII–XIV Centuries," *Cahiers du monde russe et soviétique*, 19 (1978): 401–21; Thomas S. Noonan, "Medieval Russia, the Mongols, and the West: Novgorod's Relations with the Baltic, 1100–1350," *Mediaeval Studies*, 37 (1975): 316–39; and Thomas S. Noonan, "Russia's Eastern Trade, 1150–1350: The Archeological Evidence," *Archivum Eurasiae Medii Aevi*, 3 (1983): 201–64; the studies by Lawrence N. Langer, "The Russian Medieval Town: From the Mongol Invasion to the End of the Fifteenth Century" (Ph.D. Dissertation, University of Chicago, 1972); "The Medieval Russian Town," *The City in Russian History*, ed. M. Hamm (Lexington, Ky., 1976), 11–33; "The Black Death in Russia: Its Effect upon Urban Labor," *Russian History*, 2 (1975): 53–67; and "Plague and the Russian Countryside: Monastic Estates in the Late Fourteenth and Fifteenth Centuries," *Canadian-American Slavic Studies*, 10 (1976): 351–68. On Novgorod, V. L. Ianin, *Novgorodskaiia feodal'naia votchina* (Moscow, 1981); M. W. Thompson, *Novgorod the Great* (London, 1967); and B. A. Kolchin and V. L. Ianin, *Arkheologicheskoe izuchenie Novgoroda* (Moscow, 1979).

⁴ Charles J. Halperin, *Russia and the Golden Horde* (Bloomington, Ind., 1985), esp. 75–86; V. T. Pashuto, B. N. Floria, A. L. Khoroshkevich, *Drevnerusskoe nasledie i istoricheskie sud'by vostochnogo slaviansstva* (Moscow, 1982). The Rus', of course, built prolifically with wood, which was near at hand and plentiful even in southern Rus' before the Mongol conquest. Ordinary people had the skills to work it quickly and cheaply. While the amount of building in wood might be taken as a guide to population, it need not correlate with economic trends. Even if it did, the ephemeral nature of wooden structures at most sites makes them useless as units of measure.

icans, and Incans, for instance, commonly built with stone; the Chinese, except in some fortifications, rarely built masonry walls (and used brick more for screening than for weightbearing walls); the Japanese built in wood.⁵

With the advent in the 1340s of the Black Death, the relationship between economic change and building in Europe was no longer obvious. Indeed, the coincidence of plague, economic ruin, and other woes alongside striking examples of economic vitality of certain regions, towns, and classes in the late fourteenth and fifteenth centuries has generated a variety of historical interpretations.⁶ The sharp contrasts that characterize European economic and social history in this period, however, may not be as damaging to this method of tracing economic trends as they might seem. In the studies of towns and regions of late medieval Europe, we find that, where there was prosperity, there was building. Conversely, bad times reduced the amount of building or caused it to be suspended entirely. Pipe and fabric rolls of building projects provide unambiguous evidence that economic instability was the primary reason why the construction time of Gothic cathedrals, punctuated as it often was with periods when nothing was built, extended over many generations. The cycles of construction, collapse, repair, and expansion of the Troyes Cathedral in the thirteenth century and after are a striking example of building mirroring economic trends. The episodic construction of town walls also continued to be a barometer of changing economic conditions. And, when we look at who built the magnificent town houses, palaces, and churches, not to mention the imposing fortifications of the age, we find records of families, groups, and regions that produced or controlled Europe's wealth. Finally, it should be noted that the coincidence of building with economic prosperity, and its abeyance in depressed

⁵ Miskimin, *Economy*, 21–22; Harry A. Miskimin and R. S. Lopez, "The Economic Depression of the Renaissance," *Economic History Review*, 2d ser., 14 (1962): 408–26, on the expansion of walls; Fritz Rörig, *The Medieval Town* (Berkeley, Calif., 1967), 139–40; Robert Gottfried, *Bury St. Edmunds and the Urban Crisis: 1290–1539* (Princeton, N.J., 1982), 14–45; Colin Platt, *The English Medieval Town* (London, 1976), 41–44, 59–60, 152–59, 171–74, mention monumental building as illustrative of economic growth in medieval Europe. See John K. Fairbank and Edwin O. Reischauer, *East Asia: The Great Tradition* (Boston, 1958), 48 and following; Joseph Needham, *Science and Civilization in China*, vol. 4, pt. 3 (Cambridge, 1971), 38–208; George B. Sansom, *Japan, A Short Cultural History*, rev. edn. (New York, 1962), 145–56, 186–92, 250–52, on China and Japan. On the Americas, see John Edwin Fagg's survey and bibliography, *Latin America*, 3d edn. (New York, 1977), 4–31, 808–09; on the Middle East and Mediterranean, Chester G. Starr, *A History of the Ancient World*, 2d edn. (New York, 1974), 31 and following; J. Boardman, J. Griffen, O. Murray, eds., *The Oxford History of the Classical World* (London, 1986), 283–86, 298–99, 496–502, 771–99; relevant chapters of John Fitchen, *Building Construction before Mechanization* (Cambridge, Mass., 1986).

⁶ Robert Gottfried, *The Black Death: Natural and Human Disaster in Medieval Europe* (New York, 1983); Gottfried, historiographical review in *Bury*, 8–13; Abel, *Agricultural Fluctuations*, 35–95; Joan Thirsk, "The Rural Economy," *Our Forgotten Past*, ed. Jerome Blum (London, 1982), 84–85; Miskimin, *Economy*, 25–170, on the fourteenth to fifteenth-century crisis. Gottfried, *Bury*, 35–95; David Herlihy, *Medieval and Renaissance Pistoia* (New Haven, Conn., 1967); David Herlihy, "The Distribution of Wealth in a Renaissance Community: Florence, 1427," and Charles Phythian-Adams, "Urban Decay in Late Medieval England," in *Towns and Societies: Essays in Economic History and Society*, eds. Philip Abrams and E. A. Wrigley (New York, 1978), 131–86; Philip Benedict, "Late Medieval and Early Modern Urban History," *Comparative Studies in History*, 28 (1986): 169–80; D. M. Nicholas, *Towns and Countryside in Fourteenth-Century Flanders* (Brugge, 1971); Lauro Martines, *Power and Imagination: City States in Renaissance Italy* (New York, 1979), 162–84; Richard Goldthwaite, *Private Wealth in Renaissance Florence* (Princeton, N.J., 1968); Richard Goldthwaite, *The Building of Renaissance Florence* (Baltimore, Md., 1980), 29–66, 318–50, are studies of local changes.

areas or bad times, correlates well with other indicators of general economic trends.⁷ Monumental building, therefore, ought to serve as a useful indicator of economic trends, whether by region or over time, in those cases for which ancillary evidence is lacking, as for Rus'. If matters of building materials, size, and other evidence bearing on costs, and information about patronage are also considered, it is possible to compare economic trends in different regions and to comment on their cultural and political significance.

Northern Rus', the lands of Novgorod (including Pskov), Suzdalia, and Riazan', was ruled by the Mongols directly or indirectly for two centuries after the invasion of 1237–1238 and became the core of the Muscovite Russian state (Figure 1).⁸ This study compares the amount of masonry and brick construction there with the total recorded throughout Rus' in the century before the invasion in order to measure the relative economic vitality of northern Rus' (Figure 2). It presents construction totals by period for northern Rus' from 1138 to 1462 (Figure 3) to examine economic effects of the conquest and the subsequent recovery, with factors such as building size included in order to make statistical results more meaningful. Estimates of cost will be useful in assessing Figure 4, which breaks down construction totals in northern Rus' by principality, 1238–1462. With this information, I shall compare economic changes and patronage patterns and comment on their relevance to political struggles in northern Rus' in which Moscow emerged triumphant.

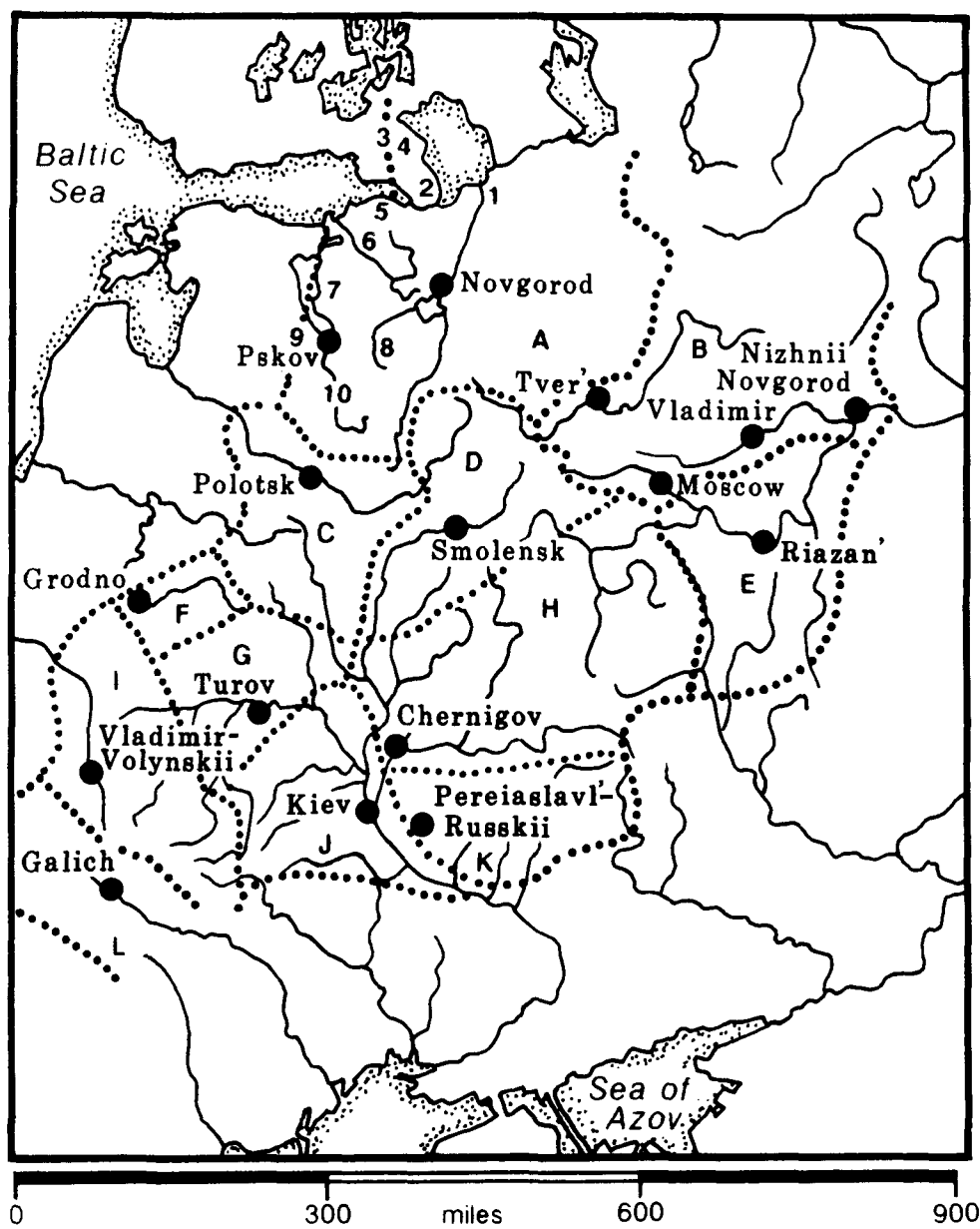
THE DATABASE RECORDS 483 CONSTRUCTION PROJECTS of stone or brick in Rus' as a whole during the period 1138–1237, and, in the north, 1238–1462. Each construction unit has equal weight for statistical purposes because 389 (or 81 percent) of the buildings are of the same sort: churches or chapels. Forts obviously were more costly to build, and their significance will be considered separately. Finally, the study counts the reconstruction of buildings and the extension of walls as independent records. Where a comparison of the size of structures is desirable, I provide the measure of foundations (in square meters); the heights are, of course, known only for the structures that survive. To the extent that it is possible, I shall also compare the size of forts.⁹ By combining this

⁷ Fitchen, *Building Construction*, 17, 49, makes the general connection between building and economics. See Goldthwaite, *Building*, 1–30, 67–112, 397–400, on Renaissance Italy; Gottfried, *Bury*, 14–45; and Platt, *English Medieval Town*, 171–74, 183–86, on economics and building in England; and Stephen Murray, *Building Troyes Cathedral* (Bloomington, Ind., 1987), 1–109.

⁸ I interpret "Rus'" broadly to encompass the totality of lands in pre-Mongol times said to have been the patrimony of the Riurikid dynasty. P. A. Rappoport, *Russkaia arkhitektura X–XIII vv. Katalog pamiatnikov*, AN SSSR, In-t. Arkheologii, *Arkheologiya SSSR. Svod arkheologicheskikh istochnikov*, no. E1–47 (Leningrad, 1982).

⁹ The database includes 377 churches, twelve chapels, three refectories, eighteen walls, nineteen forts, sixteen kremlins (or segments of), thirteen towers, eight palaces, five gates, one column, one baptistry, and five miscellaneous and five unidentified structures. Voronin, *Zodchestvo*, 2: 228–34, described masonry construction in northern Rus'; see below for my use of his estimate of labor and fabric needed to build the Moscow kremlin. We are better informed about construction materials, methods, and costs of building in other regions of Europe; Douglas Knoop and G. P. Jones, *The Mediaeval Mason*, 3d edn. (Manchester, 1967), 2–63; Louis Salzman, *Building in England down to 1540* (Oxford, 1967), 119–48; Goldthwaite, *Building*, 115–241.

Figure 1
THE LANDS OF RUS' ACCORDING TO P. A. RAPPAPORT



LANDS		FORTS	
A	Novgorod	1	Ladoga
B	Suzdalia	6	Iamgorod
C	Polotsk	2	Oreshek
D	Smolensk	7	Gdov
E	Riazan'	8	Porkhov
F	Chernaia Rus'	9	Izborsk
G	Turov	10	Ostrov
H	Chernigov		
I	Volyn'		
J	Kiev		
K	Pereiaslavl'		
L	Galich		

information with data about building materials, quarry location, construction time, labor recruitment and wages, it is possible to make (even cruder) comparisons of construction costs. Patronage data exist for 57 percent of the construction records for northern Rus' from 1238 to 1462, and probable builders have been identified for an additional 14 percent. Although incomplete, the information greatly enhances historical knowledge of the distribution of wealth and, from this, our understanding of politics and the values that might have caused the powerful to invest in buildings of one sort over another, or in buildings over alternative investments.

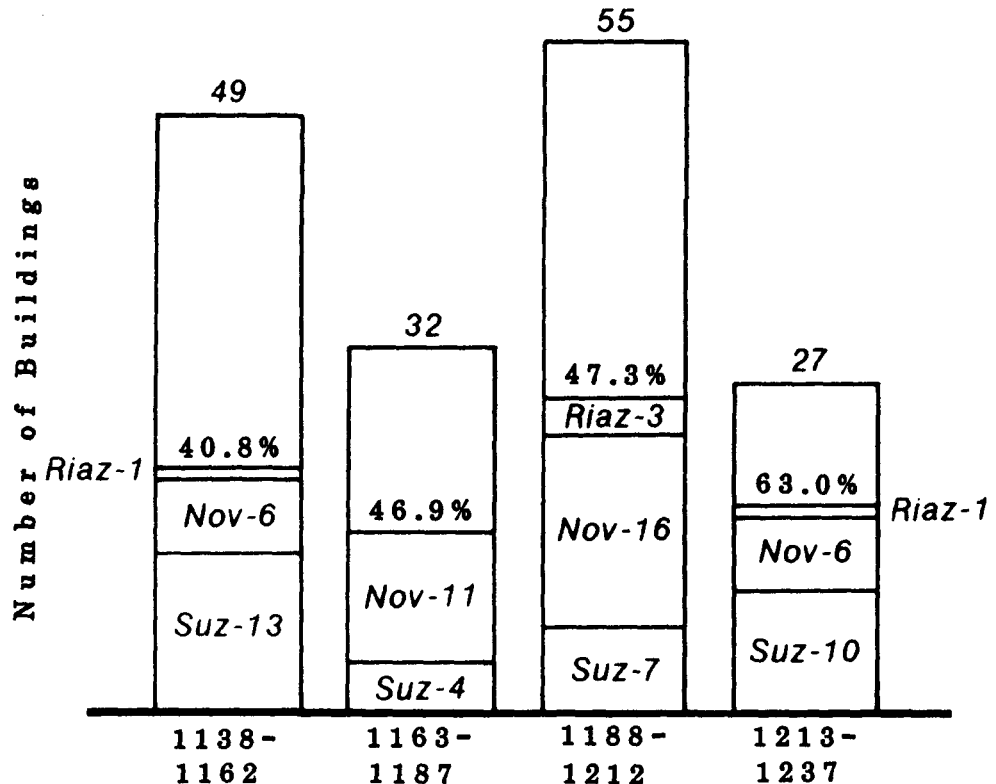
Because the starting date of a construction project appears in the sources more often (298 times) than the date of completion, I have used the beginning date as the reference point in making the graphs. In forty-five instances in which only the year a building was completed is available, this date is entered instead, the justification being that the time-span between beginning and completion was three years or less in 87 percent of the records for which we have both dates.¹⁰ If this relationship held true in most of the incomplete records, the method will not distort the calculations or their significance. Unfortunately, in the case of 140 other structures, only archaeological evidence survives. Although the majority of these have been dated to middle of the x century, or to the end of x century and beginning of $x + 1$ century, I have entered the "date begun" as a specific year, it being the mean date for the estimated period in which the structure was built.¹¹ The study minimizes distortions that might arise from this method by the manner in which it groups records into time periods. The starting point is to make 1237/1238 a dividing line so as to focus on the impact of the Mongol invasion on northern Rus'. From this divide, I have worked backward and forward, grouping construction by twenty-five-year periods. This allows me to measure short-run changes but also to validate in part an estimate such as "mid-twelfth century" by placing it in the middle of one of the quarter-century periods. It is also fortuitous, but appropriate, that by this method the investigation ends in 1462, the year in which Ivan III inherited a united Grand Principality of Moscow and Vladimir, one to which Novgorod owed vassalage and over which the Mongols no longer held power.

BUILDING RECORDS provide new information about economic trends in late pre-Mongol Rus'. Allowing for the distortion in Figure 2 resulting from the clustering of estimated records under 1150 and 1200, it is apparent that

¹⁰ Two-thirds were begun and finished in the same year; another 13 percent were completed in the following year, and 8 percent more within two years.

¹¹ Salzman, *Building*, vi, maintained that, with adequate evidence, structures can be dated with a margin of error of twenty years. The method of recording estimates tends to cluster dates under 1150, 1200, and other "round numbered years." This distortion has little effect and shows up only in Figure 1 and to a lesser extent in the first part of Figure 2. This is because well over two-thirds of dates estimated exclusively from archaeological evidence relate to building throughout Rus' before 1238; also, only fifty-five such dates during the period 1138–1462 pertain to structures in northern Rus', and they tend not to cluster.

Figure 2
MONUMENTAL BUILDING IN NOVGOROD, SUZDALIA, AND RIAZAN'
COMPARED TO TOTAL BUILDING IN RUS', 1138-1237
BY 25 YEAR PERIODS



southern Rus' experienced a steady rate of construction in the century before the Mongol invasion. This trend is in line with recent studies indicating that Kiev remained the largest city in Eastern Europe north of the Danube, that Galich and possibly Chernigov were growing towns, and that town formation in Volyn' appears to have been vigorous, bringing into question the traditional view that southern Rus' was in economic decline, whether as a result of princely warfare, nomadic incursions, or a trade slump.¹² Building records for western lands of

¹² The database records seventeen masonry and brick projects in Kiev, three in Pereiaslavl'-Russkii, eight in Chernigov, and fourteen in Galich-Volyn', 1138-1237. Because towns other than Kiev in Ukraine have not been investigated thoroughly, the database may understate totals for southern Rus'. Pereiaslavl'-Russkii was the exception. By 1187, it was a dependency (and one of the least important, judging by inheritance patterns) of the ruling house in Suzdalia. John Fennell, *The Crisis of Medieval Russia, 1200-1304* (London, 1983), 10-11. Because of this and perhaps Polovetsian raids, no buildings are known to have been built there after the 1150s. P. P. Tolochko, *Kiev i Kievskaiia zemlia v epokhu feodal'noi razdroblennosti XII-XIII vekov* (Kiev, 1980); V. I. Mezentsev, "Pro formuvan-

Chernaia Rus', Turov, and Polotsk suggest prosperity until about 1190, after which construction almost ceased. Smolensk built as many buildings as Galich-Volyn' and Polotsk combined, although construction seems also to have dropped precipitously in the thirteenth century.¹³ The strong upward curve of construction in Novgorod is in line with other evidence of growing prosperity. However, as was true for other western lands, building may have slackened in the final quarter-century, quite likely the result of a shared experience of economically exhaustive warfare with Lithuanian tribes and various German crusading orders. The pattern for Suzdalia and Riazan' was relatively stable and strong throughout. It is important as well to realize that emerging towns in the mid-twelfth century built big churches, all but one being episcopal sees, that were comparable to the cathedrals built in Kiev, Novgorod, and elsewhere about a century earlier. Prince Andrei Bogoliubskii no doubt consciously built the central cupola of the Church of the Dormition in Vladimir to be higher (32.3 meters) than that of St. Sophia in Kiev (28.3 meters).¹⁴

Finally, Figure 2 shows that building in the northern lands of Novgorod, Suzdalia, and Riazan' increased not only over time but also as a percentage of total building in Rus'. The increase was significant, from 41 percent during 1138–1162 to 63 percent during 1213–1237. Furthermore, the proportion of construction in northern Rus' rose consistently through each of the quarter-century time periods. From this information, it appears likely that northern Rus' was growing in wealth and possibly in population more rapidly than Rus' as a whole before the Mongol invasion.

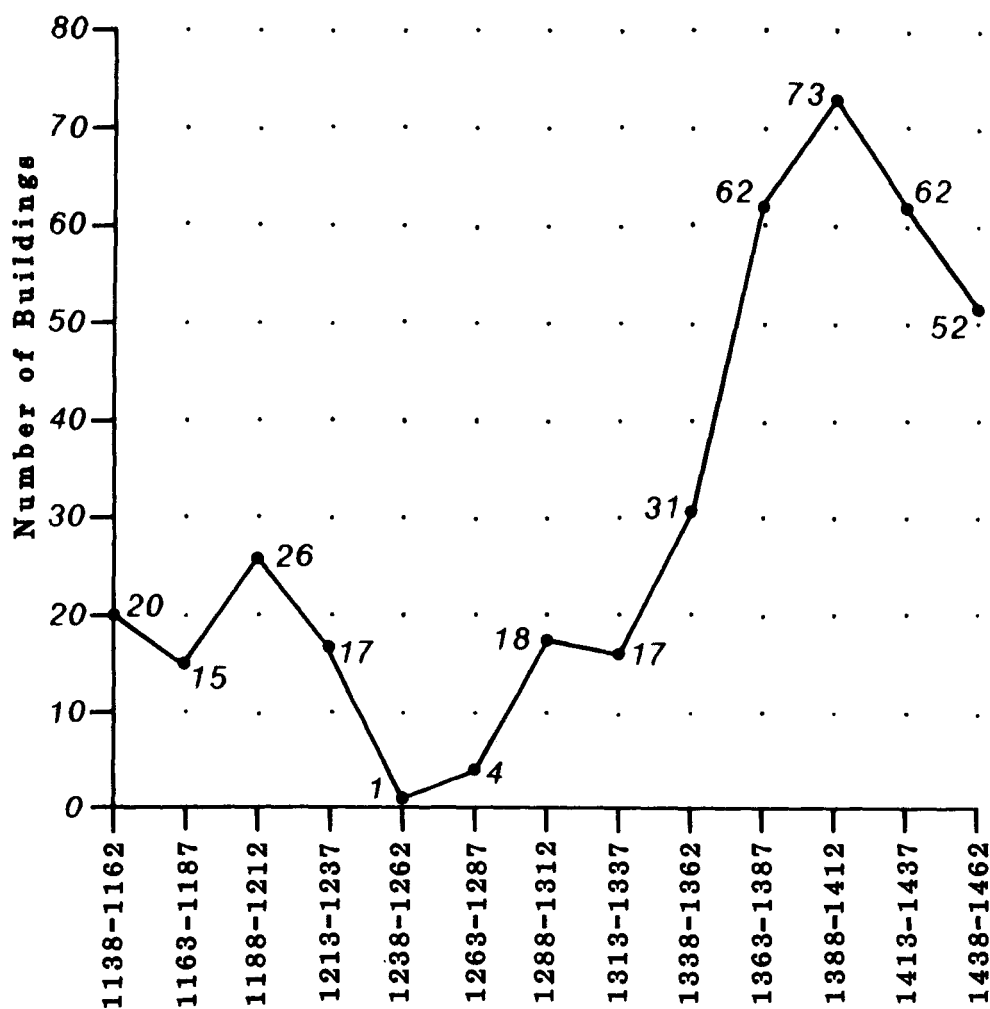
FIGURE 3 ILLUSTRATES THE SEVERITY AND THE TENACITY of the depression in monumental construction that followed the Mongol invasion. Northern Rus' had enjoyed an ascending rate of growth in building in the century before the invasion. As many as twenty-six monumental buildings were built in northern

nia mis'koi terytorii davn'oho Chernihova," *Arkheolohiia*, 34 (1980): 53–64; V. I. Mezentssev, "The Territorial and Demographic Development of Medieval Kiev and Other Major Cities of Rus': A Comparative Analysis Based on Recent Archeological Research," forthcoming in *The Russian Review*; Thomas Noonan, "The Transformation of Kiev into a Major European Commercial and Industrial Center during the Pre-Mongol Era," unpublished paper read at the Meeting of the American Association for the Advancement of Slavic Studies, New Orleans (November 1986); David B. Miller, "The Kievan Principality in the Century before the Mongol Invasion: An Inquiry into Recent Research and Interpretation," *Harvard Ukrainian Studies*, 10 (1986): 215–40, argue that southern Rus' remained prosperous to 1240.

¹³ For the four quarters before 1238, the totals for Chernaia Rus' are 5, 3, 0, 0 respectively; for Polotsk: 5, 4, 1, 0; for Smolensk 7, 3, 11, 3.

¹⁴ Compare foundation sizes of the Church of the Dormition in Galich (ca. 1150): 1,215 sq. meters; that in Vladimir-Volyn'skii (1156–60): 715 sq. meters; that in Rostov (1161–62, reconstructed 1213–31): 954 sq. meters; that in Vladimir in Suzdalia (1185–89): 1,183 sq. meters; the Church of Sts. Boris and Gleb in Riazan' (1194): 632 sq. meters, with St. Sofia, Polotsk (ca. 1050): 832 sq. meters; St. Sofia, Kiev (ca. 1050): 2,299 sq. meters with galleries; St. Sofia, Novgorod (1046–50), with galleries: 1,348 sq. meters; Church of Our Savior, Chernigov (ca. 1050): 734 sq. meters; Church of St. Michael the Archangel, Pereiaslavl'-Russkii (completed 1089; rebuilt twice before 1238): over 845 sq. meters; Rappoport, *Russkaia arkhitektura*, nos. 10, 44, 55, 70, 74, 91, 97, 161, 183, 187; Voronin, *Zodchestvo*, 1: 189, 316.

Figure 3
MONUMENTAL BUILDING IN NORTHERN RUS', 1138-1462
BY 25 YEAR PERIODS



Rus' in a single quarter-century (1188-1212); in no quarter-century were there fewer than fifteen (1163-1187). Measured against this level, the fifty years after the conquest were nothing less than a calamity. In the first quarter-century after 1237, Bishop Kirill of Rostov restored the small Church of Sts. Boris and Gleb in Kideksha near Vladimir, the only work done in stone or brick.¹⁵ Novgorod,

¹⁵ *Polnoe sobranie russkikh letopisei* [hereafter, *PSRL*] (St. Petersburg-Petrograd-Leningrad-Moscow, 1846- , and 2d edn.; St. Petersburg-Leningrad, 1908), 1: 469; Rappoport, *Russkaia arkhitektura*, no.

although it had entirely escaped destruction, built nothing. For the next twenty-five-year interval, 1263–1287, there are records of only four projects, two in Novgorod and two in Suzdalia, of which one was a reconstruction. For the next fifty years, 1288–1337, the pace of construction quickened, and there is evidence of thirty new or rebuilt structures. Yet the total is well below that for the fifty years before the invasion.

If Novgorod and Suzdalia experienced a century-long economic depression, the Riazan' land must have become a primitive frontier. Although the fourteenth-century "Tale of the Destruction of Riazan'" told of princes building churches and monasteries after the sack of 1237, in fact, Riazan' became a ghost town. In the fourteenth century, its princes reestablished themselves fifty kilometers to the west in Pereiaslavl'-Riazanskii. It has been suggested without much proof that the Church of the Dormition in the Riazan' kremlin and a gate tower there might have been built of stone sometime after 1390, but neither structure has been studied carefully. Excepting these, there is no evidence of masonry or brick building in the Riazan' land prior to 1462.¹⁶

Construction records do not explain why northern Rus' remained stagnant; they only suggest that Mongol tribute, levies of labor, punitive expeditions, and other impositions, following on the destruction of the initial invasion, siphoned off resources and skilled workers on a grand scale. Although the specific burden of these hardships cannot be known from surviving evidence, the sorry record of monumental building should cause historians to reject interpretations that would minimize the economic consequences of the conquest.¹⁷

The earliest signs of economic recovery were construction of a major fort (Kopor'e, 1280) and a masonry church in Novgorod (St. Nicholas, 1292), and the resumption of monumental building in Suzdalia (Church of the Transfiguration, Tver', 1285–1290).¹⁸ Although building, especially of costly forts, could

85; Voronin, *Zodchestvo*, 2: 129–34; N. S. Borisov, "Russkaia arkhitektura i mongolo-tatarskoe igo (1238–1300)," *Vestnik Moskovskogo universiteta*, series 9: *Istoriia* (1976), no. 6, 63–79; Rybakov, *Remeslo*, 665–75.

¹⁶ N. V. Ponyrko, ed., *Voinskie povesti drevnei Rusi* (Leningrad, 1985), 105; A. L. Mongait, *Staraia Riazan': Materialy i issledovaniia po arkheologii SSSR* [hereafter, *MIA*], 49 (Moscow, 1955), 27–28. A seventeenth-century source described Pereiaslavl'-Riazanskii's kremlin as having a gate tower of stone and brick from which V. V. Kostochkin, "Voenno-oboronitel'nye sooruzheniia," *Ocherki russkoi kul'tury XIII–XV vekov*, 2 vols., A. V. Artsikhovskii, et al., eds. (Moscow, 1969), 1: 448, tentatively dated the kremlin to the second half of the fourteenth century. It is unlikely that brick would have been used then, and Kostochkin offered no further evidence of when the kremlin was built or whether the gate tower was an original part of it. G. K. Vagner, *Riazan'* (Moscow, 1971), 16; and E. V. Mikhailovskii, *Riazan'. Pamiatniki arkhitektury i iskusstva* (Moscow, 1985), 24–26, dated the original Church of the Dormition between about 1390 and 1427. Fennell's optimistic view of the economic health of Riazan' before 1300, *Crisis*, 88, seems overstated.

¹⁷ Northern Rus' also had to summon resources to oppose military threats to Novgorod from the Teutonic Order and Sweden; Eric Christiansen, *The Northern Crusades* (Minneapolis, Minn., 1980), 105–31; I. P. Shaskol'skii, *Bor'ba Rusi protiv krestonosnoi agressii na beregakh Baltiki v XII–XIII vv.* (Leningrad, 1978), 122–233. While the Mongols excluded the Rus' from Volga-steppe commerce, Novgorod merchants obtained safe passage across Suzdalia to the east in 1270; *Gramoty Velikogo Novgoroda i Pskov*, ed. S. N. Valk (Moscow, 1949), no. 3, 12–13; Noonan, "Russia's Eastern Trade," 213 and following; Langer, "The Medieval Russian Town," 16–17; Martin, "Land of Darkness," 405–07, 409–16.

¹⁸ *Novgorodskaia pervuaia letopis'* [hereafter, *NPL*], ed. A. N. Nasonov (Moscow, 1950), 323, 327; *Troitskaia letopis': Rekonstruktsiia teksta* [hereafter, *TL*], ed. M. D. Priselkov (Moscow, 1950), 343–45.

only indirectly be said to promote economic growth or well-being, it was impressive testimony that northern Rus' again had begun to generate wealth. At first, regular building only took place in the Novgorod land. But, by the 1320s, a concerted record of building appears in Suzdalia as well. The boom continued for eighty years and included major projects requiring large investments of human resources and wealth. In 1302, work commenced in Novgorod on a *detinets* (kremlin) of stone and brick. Beginning in 1335, the "Trade Side" across the river was partially fortified with a stone wall, and soon stone towers appeared on Novgorod's outer walls of earth and wood. In 1309, a wall was built to protect Pskov's commercial and residential section (*posad*) and was extended many times. In 1337, Pskov began to wall its kremlin with stone.¹⁹ Novgorod and Pskov also built and continually strengthened a string of forts along the western frontier. The early forts at Izborsk, west of Pskov (1303), and Oreshek on Lake Ladoga at the source of the Neva River (1323), at first little more than stone keeps, soon became substantial walled forts with towers (1330; 1352 and 1410). By the 1400s, Pskov and Novgorod had six more forts: Kopor'e in Izhora (rebuilt in 1297, expanded in 1365), Tiverskii gorodok and Korela (Keksholm) in Karelia (approximately 1338, 1364), Iamgorod on the Luga River (1384), Ostrov on the Velikaia River (approximately 1350; rebuilt approximately 1386), and Porkhov on the Shelon (1387).²⁰ In Suzdalia, Grand Prince Dmitrii Donskoi built a stone kremlin in Moscow in 1367–1368, and in 1365 Prince Boris Konstantinovich began work on a stone kremlin in Nizhnii Novgorod. Work resumed in 1372, but

¹⁹ *NPL*, 331, 343, 345–46, 372, 385, 396; *PSRL*, 4, pt. 1: 370, 390; 15, pt. 1 (2d edn.): 72; 16: 90; *Pskovskie letopisi*, 2 vols., ed. A. N. Nasonov (Moscow, 1941–55) 1: 14, 17, 23–26; 2: 22, 24, 28–31, 92, 106–08. On Novgorod: M. Kh. Aleshkovskii, L. E. Krasnorech'ev, "O datirovke vala i rva Novgorodskogo ostroga," *Sovetskaiia Arkheologiia* (1970), no. 4, 54–73; and "K datirovke vala i rva Novgorodskogo ostroga. Otvet S. N. Orlovu," *Sovetskaiia Arkheologiia* (1972), no. 3, 392–95; M. Kh. Aleshkovskii, "Novgorodskii detinets 1044–1430 gg.," *Arkhiturnoe nasledstvo*, 14 (1962): 1–26; Henrik Birnbaum, *Lord Novgorod the Great* (Columbus, Ohio, 1981), 59–60; V. L. Ianin, "O prodolzhitel'nosti stroitel'stva Novgorodskogo kremliia XV v.," *Sovetskaiia Arkheologiia* (1978), no. 1, 259–60; M. K. Karger, *Novgorod velikii* (4th edn.; Moscow, 1980), 100–02; and A. L. Mongait, "Oboronitel'nye sooruzheniia Novgoroda Velikogo," *MIA*, 31 (1952): 7–132, arguing that the kremlin had stone walls before 1237. For Pskov: M. Kh. Aleshkovskii, "Nachal'nye etapy kamennogo stroitel'stva Pskovskogo kroma," *Drevnerusskoe iskusstvo. Khudozhestvennaia kul'tura domongol'skoi Rusi* (Moscow, 1972), 322–48; P. A. Rappoport, "Persi Pskovskogo kroma," *Kratkie soobshcheniia In-ta istorii material'noi kul'tury AN SSSR*, 62 (1956): 56–58; I. K. Labutina, *Istoricheskaia topografiia Pskova v XIV–XV vv.* (Moscow, 1985), 31–61. I. K. Labutina, "Letopisnye dannye XIV v. o krepостnykh sooruzheniiakh Pskova," *Sovetskaiia Arkheologiia* (1970), no. 2, 93–108; V. D. Beletskii, "Arkheologicheskie dannye k datirovke krepостnykh sten Dovmotova goroda," *Arkheologicheskii sbornik Gos-ogo Ermitazha*, 12 (1970): 68–80, argued that Pskov had stone walls before 1300.

²⁰ *NPL*, 100, 328, 339, 379, 381, 475–77; *Pskovskie letopisi*, 1: 14, 17 and 2: 92; *PSRL*, 4, pt. 1: 288, 410; 5: 202; 16: 57, 90, 132, 159; M. N. Tikhomirov, "Spisok russkikh gorodov dal'nikh i blizhnikh," in his *Russkoe letopisanie* (Moscow, 1979), 130; A. N. Kirpichnikov, *Kamennye krepости novgorodskoi zemli* (Leningrad, 1984); P. A. Rappoport, "Iz istorii voennoinzhenernogo iskusstva drevnei Rusi (Staraia Ladoga, Porkhov, Izborsk, Ostrov)," *MIA*, 31 (1952): 133–201; P. A. Rappoport and V. V. Kostochkin, "K voprosu o periodizatsii istorii drevnerusskogo voennogo zodchestva," *Kratkie soobshcheniia In-ta istorii material'noi kul'tury AN SSSR*, 59 (1955): 25–27; V. V. Kostochkin, "Kreml' drevnego Gdova," and "K kharakteristike pamiatnikov voennogo zodchestva Moskovskoi Rusi kontsa XV–nachala XVI vekov," *MIA*, 77 (1958): 96–97, 120, 122–25, 134–35; V. V. Kostochkin, "O datirovke krepостei Ostrova i Izborska," *Kratkie soobshcheniia In-ta istorii material'noi kul'tury AN SSSR*, 62 (1956): 59–65; V. V. Kostochkin, "Stroitel'naia biografiia kreposti Izborska," *Sovetskaiia Arkheologiia* (1959), no. 1, 124–43; V. V. Kostochkin, "Voенно-oboronitel'nye," 416–30; and A. R. Artem'ev, "Stratigrafiia i khronologiia izborskoii kreposti," *Sovetskaiia Arkheologiia* (1985), no. 2, 130–40.

it was never finished.²¹ Nevertheless, the increasing tempo and scale of construction in northern Rus' suggests that there was substance to the perception of the Muscovite chronicler who, in the entry for 1328, wrote that, when Ivan Kalita of Moscow became Grand Prince, he inaugurated "a long peace of forty years, [in which] pagans ceased to war on the Russian land and [ceased] to slaughter Christians . . . , and thenceforth a great peace reigned throughout the land."²²

In preindustrial societies, natural and political calamities could alter economic trends dramatically. It is therefore remarkable that various afflictions lamented by chroniclers after 1300, the most serious of which were repeated epidemics of plague, had no measurably adverse effect on the building boom. Plague visited Novgorod and Pskov in 1352–1353, struck again in 1360, and spread to Suzdalia. Thereafter it became endemic, as was the case throughout Europe, reappearing each decade until 1448.²³ Descriptions of chroniclers suggest that it also had the same ghastly effects on Russian towns as on those further west, killing with frightful efficiency. While plague must have depressed economic growth, one can do little more than speculate about why it did not cause a decline in monumental building in the fourteenth century as it did in Western and Central Europe. Even when the number of structures built each quarter-century in northern Rus' began to drop after 1412, it is difficult to connect the slump to economic woes brought on by plague. Novgorod, for example, in the period 1413–1437, recorded its highest total of projects, including several large ones. To explain this anomaly, historians have suggested that chroniclers exaggerated, that plague was less devastating in northern Rus'. It may be that a decline in building does not show up because the upswing in construction had begun from so low a level that retarding effects of plague still left northern Rus' with a net gain. It could also be argued that high death rates concentrated wealth in fewer hands or that the dying left their wealth to the church, in either case making construction more possible. Research for this article and analogy from European

²¹ *TL*, 384, 394; *PSRL*, 15, pt. 1 (2d edn.): 74, 100; 23: 114; *Nizhegorodskii letopisets*, ed. A. S. Gatsiskii (Nizhnii Novgorod, 1886), 12–17; Voronin, *Zodchestva*, 2: 175–79, 213–14; N. N. Voronin, "Moskovskii kreml' (1156–1367 gg.)," *MIA*, 77: 52–66; I. V. Trofimov and I. A. Kir'ianov, "Materialy k issledovaniiu Nizhegorodskogo kremlia," *MIA*, 31: 318–46. Aleshkovskii, "Nachal'nye etapy," 347, questioned whether Dmitrii's kremlin was entirely of stone, noting the short time of construction, the likelihood that construction of kremlins usually proceeded in stages, and because Ambrogio Contarini, who visited Moscow in 1476–77, said it was of wood. Also Joel Raba, *The Moscow Kremlin: Mirror of the Newborn Muscovite State* = The Russian and East European Research Center, Tel Aviv University, *Slavic and Soviet Series*, 2 (1975): 6–7; and *Barbaro i kontarini o Rossii*, ed. E. Ch. Shzhinskaiia (Leningrad, 1971), 203–04. I am unaware of evidence of a Mongol prohibition against fortifications, although a century earlier (1259) in southern Rus' the Mongol commander Burundai compelled Princes Danilo and Vasilko Romanovich of Galich and Volyn' to dismantle the walls of their towns, *PSRL*, 2: 848–52; *The Galician-Volynian Chronicle*, trans. and ed. George A. Perfecky (Munich, 1973), 78–79.

²² *TL*, 359. In fact, Ivan became Grand Prince in 1331; John Fennell, *The Emergence of Moscow, 1304–1359* (Berkeley, Calif., 1968), esp. 111–19, 190–95.

²³ *NPL*, 362–63, 367, 408; *Pskovskie letopisi*, 1: 21–23; *TL*, 379–81, 430, 438; *PSRL* 4, pt. 1: 306; 25: 212, 231, 237, 243, 244, 246–47, 270; 30: 113, 128, 131; 15, pt. 1 (2d edn.), 70, 76–77, 106; Gottfried, *Black Death*; John T. Alexander, *Bubonic Plague in Early Modern Russia* (Baltimore, Md., 1980), 11–16.

experience also suggest that survivors in Novgorod built stone churches as memorials to those struck down by plague.²⁴

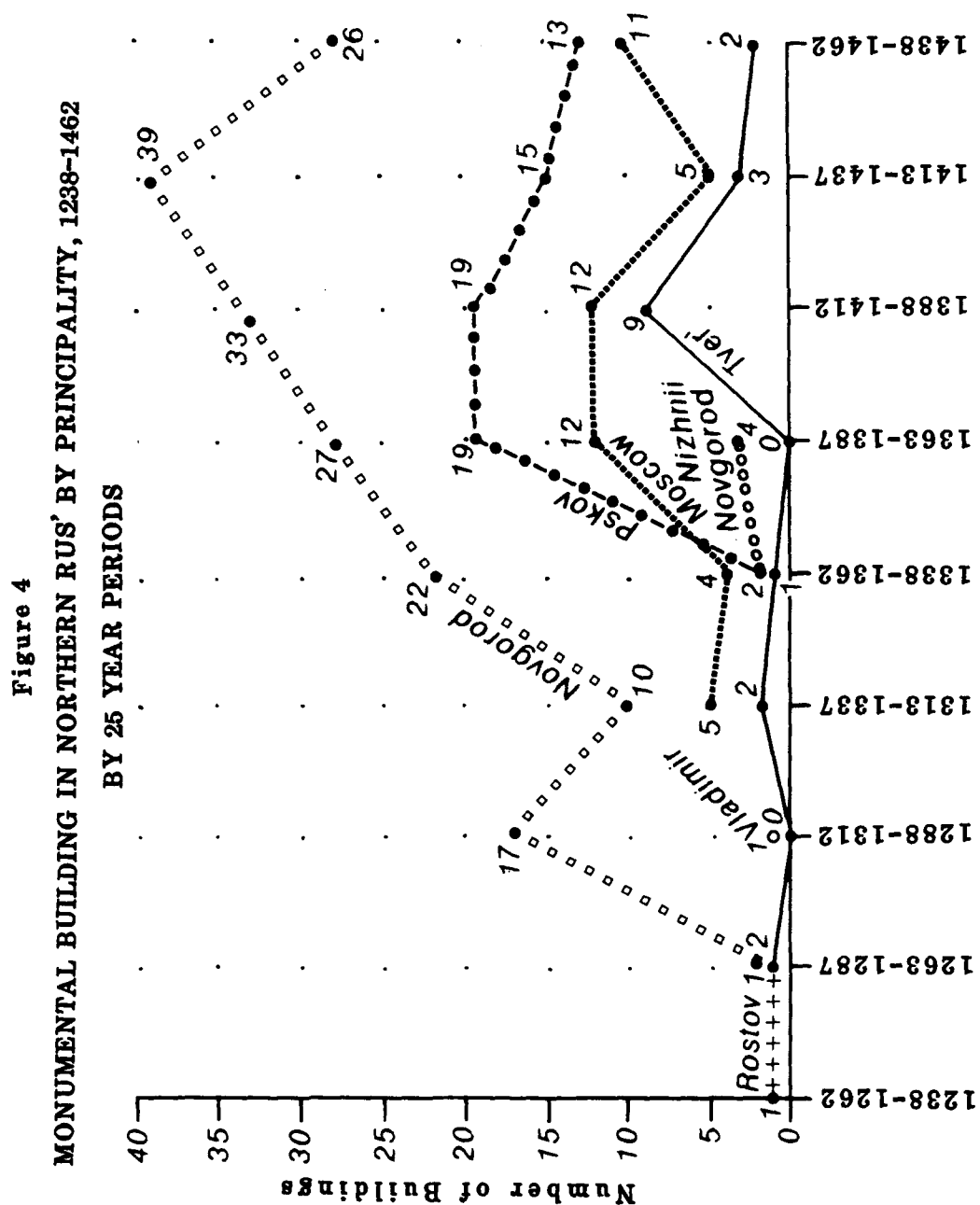
The overall increase in the number of construction projects each quarter-century to 1412 indicates that continuous fighting with Lithuanian and Tatar armies from the 1370s, including Khan Tokhtamysh's sack of Moscow and devastation of Suzdalia and Riazan' in 1382 and the burdens that followed, was considerably less destructive to the economy of northern Rus' than the original conquest.²⁵ This does not mean that individual principalities in Suzdalia did not suffer economically. Tver', for instance, was unable to afford to build at all between 1353 and 1394, as (losing) wars devastated its economy numerous times. The principality must have recovered rapidly, however, because, in little over a decade, 1394–1407, the people of Tver' erected nine churches. Figure 4 shows that military disaster (discussed below) also abruptly terminated Nizhnii Novgorod's ability to build after the 1370s. Moscow, on the other hand, seems to have survived the sack of 1382 and resumed payment of a heavy tribute to the Tatars fairly well. Between 1363 and 1462, between four and ten stone structures were built in Moscow each quarter-century. In this context, the unusually long period, from 1378 to 1404, required for the Simonov Monastery to build its Church of the Dormition, despite having the grand prince and metropolitan among its patrons, was very likely caused more by changing building priorities in an inelastic economy than by a downturn. In his study of monumental building, N. N. Voronin speculated that Dmitrii Donskoi diverted resources to build walled monasteries with stone churches in Serpukhov and Kolomna as defense points on the Oka River in the late 1370s. Similarities in structural detail suggest that Moscow builders also erected two churches in Mozhaïsk and one in Zvenigorod in the 1390s.²⁶

Although monumental construction in northern Rus' proceeded vigorously from 1412 to 1462, three or four times the rate of the early fourteenth century, it nevertheless declined. At first glance, this fact seems to support Carsten

²⁴ Alexander, *Bubonic Plague*, 11–16; Langer, "Plague," 351–68. Goehrke, *Die Wüstungen*, 65–66, suggested that plague severely damaged the economy; on effects of plague on culture and building, see Goehrke, 32; and Russell Zguta, "The One-Day Votive Church," *Slavic Review*, 40 (1981): 423–32.

²⁵ *TL*, 390–428; *PSRL*, 4, pt. 1: 297–38, 406–07; 15: 430–42, 482–84; 25: 185–91, 193–96, 199–210, 238–39; 15, pt. 1 (2d edn.): 94–100, 103–07, 110–21, 133–35, 139–46, 148–49, 177–85. M. A. Salmina, "Povest' o nashestvii Tokhtamysha," *Trudy otdela drevnerusskoi literatury*, 34 (1979): 134–51; A. E. Presniakov, *The Formation of the Great Russian State* (Chicago, 1970), 248–52, 265–88; Cherepnin, *Obrazovanie*, 568–663.

²⁶ On construction in Moscow: *TL*, 381, 384, 406, 443–44; *PSRL*, 2 (2d edn.): 539; 5: 256, 274; 6: 130, 235–39; 8: 28, 149, 152, 244; 12: 75, 112–13, 158, 198, 232; 15, pt. 1 (2d edn.): 84; 18: 143, 151, 205; 22, pt. 1: 510; 23: 113–14, 133, 144, 154, 157; 25: 233, 271, 275–77; 30: 125; Voronin, *Zodchestvo*, 2: 184–86, 242–66, 326–37; L. I. Ivina, *Krupnaia votchina severo-vostochnoi Rusi* (Leningrad, 1979), 36–38, 75; Veselovskii, *Issledovaniia po istorii klassa sluzhilykh zemlevladel'tsev* (Moscow, 1969), 443–44. In Tver': Voronin, *Zodchestvo*, 2: 375–412, 425; *TL*, 449, 499; *PSRL*, 6: 132; 11: 156, 202; 15: 470, 488, 490–91, 494–95; 15, pt. 1 (2d edn.): 164–66, 186, 25: 233; Langer, "Russian Medieval Town," 88–93. In the Muscovite centers of Dmitrov, Mozhaïsk, Zvenigorod, Kolomna, Serpukhov, and the Trinity-St. Sergei Monastery: *TL*, 396–97, 419, 440; *PSRL*, 8: 31; 18: 129; Voronin, *Zodchestvo*, 2: 189–220, 267–306, 311–24, 364–66; *Pamiatniki arkhitektury moskovskoi oblasti*, 2 vols., ed. E. N. Pod'iapol'skii (Moscow, 1975), 2: 220–23; B. A. Ognev, "Uspenskii sobor v Zvenigorode na gorodke," *MIA*, 44 (1955): 20–58; in Nizhnii Novgorod: *TL*, 392, 394; *PSRL*, 8: 17; 15, pt. 1 (2d edn.): 74, 100; 23: 116.



Goehrke's view, drawn from a study of wastelands, that northern Rus' after 1400 experienced an economic depression. One might explain early stages of the slump as a result of plague or as a by-product of the Muscovite civil war. But from 1437 the rate of construction in Novgorod and Pskov fell off. No doubt Moscow's intervention in Novgorod's empire, not to mention the chronic piracy that existed on eastern trade routes with the breakup of Mongol power, hurt Novgorod's economy. Political instability may also have contributed to hard times. It is likely, however, that the gunpowder revolution was at least as important in causing the number of building starts in Novgorod and Pskov to decline. In 1428, the Teutonic Order bombarded Novgorod's border fort of Porkhov. This was the first time that artillery was used systematically against a besieged town in northern Rus'. In response, Novgorod rebuilt Porkhov, doubling the thickness of some sections of its wall to five meters at the base in 1430. In 1444 and 1447, the Order besieged and bombarded Iamgorod, compelling Novgorod and Pskov to undertake urgent countermeasures. When the Order attacked Iamgorod, they found its walls rebuilt so as to allow the besieged to fire back. In 1448, Novgorod reinforced Iamgorod's walls against artillery. It also reinforced the ancient fort at Ladoga in 1446 and its own kremlin in 1450. Pskov built a fort at Gdov in 1431, the vulnerable side of which had a stone wall. Pskov strengthened Ostrov about 1440, Izborsk about 1450, and its kremlin and walls, 1422–1452.²⁷ These projects required the labor of many masons for extended periods and thus steep increases in public spending, which could only have been met by transfers from the private sector. A decline in the construction of family and monastic churches was the result.

In sum, while there may have been a depression after 1400, it could not have been severe or have lasted very long. It is likely that economic growth resumed in northern Rus' by the 1440s, notwithstanding the decline in building starts in the Novgorod land. It remains to inquire how information about building size, materials, construction methods and costs enhance our understanding of statistical trends.

AGAIN, A LARGE MAJORITY OF THE CONSTRUCTION PROJECTS were churches, and, of these, a surprising number were completed in the year in which they were begun or within a short time thereafter. This was possible because most of them were modest in size and simple in structural detail, not only in comparison to Gothic cathedrals of Catholic Europe built at the same time but when compared to

²⁷ Goehrke, *Die Wüstungen*, 66–76. On wars with the Teutonic Order and the rebuilding of forts, see *PSRL*, 4, pt. 1: 123; 16: 185, 191–92; 25: 247–48; *NPL*, 415–16, 423; *Pskovskie letopisi*, 1: 34–5, 39, 50; N. A. Kazakova, *Russko-livonskie i russko-ganzeiskie otnosheniia* (Leningrad, 1975), 36–123. On the gunpowder revolution and its effect on fort building, see Hellie, *Enserfment and Military Change in Muscovy* (Chicago, 1971), 151–59; Kirpichnikov, *Kamennye krepasti*, 46, 54–56, 183–85, 216–62; A. N. Kirpichnikov, "Voennoe delo srednevekovoi Rusi i poiavlenie ognestrel'nogo oruzhiia," *Sovetskaia Arkheologiia* (1957), no. 3, 60–76; V. V. Kostochkin, *Russkoe oboronnnoe zodchestvo kontsa XIII–nachala XVI vekov* (Moscow, 1962), 33–34, 39; Kostochkin, "Kreml' drevnego Gdova," 67–100; Rappoport, "Iz istorii," 136–38, 162–83, 190–01; Rappoport and Kostochkin, "K voprosu," 22–28; Aleshkovskii, "Nachal'nye etapy," 333–39; Labutina, *Istoricheskaia topografiia*, 50–51.

churches of pre-Mongol Rus'. The Church of the Archangel Michael on Mikhailov Street (1300–1302), for example, with a base of 237 square meters, was the largest new church built in Novgorod after 1237. It was smaller than St. Parashkeva-Piatnitsa "in the Market" (1207), the biggest built there in the century before the Mongol invasion (some 330 square meters). It was minute compared to the Cathedral of St. Sophia in the Novgorod kremlin (1045–1050), which, even without its galleries, covered 669 square meters.²⁸ Evidence from post-1237 Suzdalia indicates that its churches were on the average only slightly larger than those of Novgorod. The first Church of the Dormition in the Moscow kremlin (1326), occupied at most 226 square meters. The largest church about which we have adequate archaeological data was the Church of the Dormition in Kolomna, about 396 square meters (1379–1382). It was the only six-column church known to have been built in the Muscovite state between 1238 and 1462. The foundation of the main church of the Simonov Monastery in Moscow (1378–1404) may have been larger, about 420 square meters, but archaeological evidence is too fragmentary to be certain.²⁹

Although mastery of brickmaking is well established for northern Rus' before the Mongol invasion, limestone and tufa were the primary building materials. Novgorod builders commonly alternated rows of stone with narrow bricks, most likely as a matter of taste. In the late twelfth century, the emergence of private patronage, demanding smaller, more quickly built, and cheaper structures caused a change in building methods, among them an increased use of local limestone, often cut in irregular sizes. In Suzdalia, esteem for "white stone" churches and the ability to build them, often with exquisite results, made brick a secondary material. When the rate of building increased after the conquest, Novgorod builders continued in the new style but at first built more simply, and more crudely, using limestone almost exclusively. Brickwork appeared, if at all, as a decorative feature framing windows and doors and on pilasters in new churches or in the reconstruction of earlier ones. Because the stone was permeable, they protected it with mortar. In Suzdalia, although little remains of structures built between 1238 and 1400, it seems that artisans forswore brick for stone altogether. There is no certain answer as to why patrons and builders of northern Rus' of the Mongol period preferred limestone to brick. Novgorodians knew how to make bricks although they made them differently than before 1238. And if Suzdalians had lost the art, it is difficult to believe that they could not have soon relearned it from their neighbors. Whatever the reason, building

²⁸ Compare Rappoport, *Russkaia arkhitektura*, nos. 97, 103; Karger, *Novgorod* (4th edn.), 138–39; and the foundation of Notre Dame. Paris (1163–1257), about 5,632 sq. meters; Jean Bony, *French Gothic Architecture of the Twelfth and Thirteenth Centuries* (Berkeley, Calif., 1983), 138.

²⁹ *TL*, 440; *PSRL*, 18: 129, 151; 25: 233; Voronin, *Zodchestvo*, 2: 152–72, 185–86, 194–203. It remains to be firmly substantiated that Prince Daniil built a stone church in Moscow in the 1280s or 1290s that was torn down in order to erect the Church of the Dormition; N. S. Sheliapina, "K istorii izucheniia Uspenskogo sobora Moskovskogo kremliia," *Sovetskaia Arkheologiia* (1972), no. 1, 200–14; T. V. Tolstaia, *Uspenskii sobor Moskovskogo kremliia* (Moscow, 1979), 64.

in limestone meant higher costs in work force, if not also in skills, and a correspondingly higher level of prosperity than had existed before 1238.³⁰

Building and improving forts, kremlins, and town walls represented investments of an altogether greater magnitude than church building, and work usually extended intermittently over many decades. There are fourteen separate references to work on Pskov's outer walls between 1309 and 1462.³¹ Its kremlin was completed in 1452. Novgorod walled its kremlin in stages by 1430, but in 1462 the town walls were still partly of wood and earth. Nizhnii Novgorod's kremlin went up in stages and was never finished. While historians know less about border forts, it is certain that some were built hurriedly in expectation of attack, a familiar story in the European Middle Ages. Novgorod supposedly built Iamgorod in thirty-three days. Izborsk was built within a year in 1330. But these and other forts were repeatedly expanded and strengthened.³² In this context, the building of the Moscow kremlin—with its circumference well over twice that of the largest fort, almost twice that of the Pskov kremlin, and greater than that of Novgorod—in little more than a year, 1367–1368, was a singular event.

The Novgorod kremlin, the Pskov kremlin and town walls, and the forts at Staraia Ladoga (rebuilding of 1446), Oreshek, Izborsk, Kopor'e, Iamgorod, and Gdov resembled one another in construction materials and methods. Typically, builders dug a foundation trench and filled it with boulders, limestone slabs, and rough stone to make a footing at least as wide as the base of the wall. Walls were built in alternating rows of granite boulders and blocks of crudely dressed limestone. Upper levels were exclusively of limestone blocks. Stone masons anchored the boulders with a mix of mortar and gravel and used mortar between layers of blocks. They filled interior spaces of thick walls with stones and rubble, solidified with mortar. The exterior walls of the fort at Porkhov and, apparently, of the Moscow kremlin (known only from written sources), however,

³⁰ See Rappoport, *Russkaia arkhitektura*, nos. 95–137; P. A. Rappoport, A. A. Peskova, and G. M. Shtender, "K voprosu o slozhenii novgorodskoi arkhitekturnoi shkoly," *Sovetskaia Arkheologiya* (1982), no. 3, 35–46; V. A. Bulkin, "Arkhiturno-stroitel'naia situatsiia v novgorodskom zodchestve kontse XII–nachala XIII vv.," *Genezis i razvitie feodalizma v Rossii. Problemy ideologii i kul'tury*, ed. I. Ia. Froianov (Leningrad, 1987), 217–23; Voronin, *Zodchestvo*, 2: esp. 142, 189, 212–13, on possible use of brick after 1238; M. K. Karger, *Novgorod* (Leningrad, 1961), 93–102 and following, and 269–70, and (4th edn.), 104, 118–20, 138–39, 159–67, 205–09, 212–16; T. V. Gladenko, *et al.*, "Arkhitura Novgoroda v svete polednykh issledovanii," *Novgorod. K 1100-letiiu goroda. Sb. Statei*, ed. M. N. Tikhomirov (Moscow, 1964), 184–25, 236, 238–39; and Rybakov, *Remeslo*, 662–75, who thought post-conquest construction in Novgorod inferior and prone to collapse. Simpler yes, but pre-conquest building was also susceptible to ruin; for instance, the Church of St. John the Baptist "na opokakh," built 1127–30, soon in ruins, rebuilt 1184.

³¹ *Pskovskie letopisi*, 1: 14, 23, 24, 26–27, 39, 51, 55, 62, and 2: 36, 115; Labutina, *Istoricheskaia topografiia*, 33–34, 46–58, 62–71; Aleshkovskii, "Nachal'nye etapy," 322–47.

³² See references in nn. 19, 20; *NPL*, 379; and *PSRL*, 4, pt. 1: 91 for Iamgorod; for Kopor'e: initial construction in 1280, *NPL*, 323; reconstruction of 1297, *NPL*, 328; and archaeological estimate for a later rebuilding (1360s), Kirpichnikov, *Kamennye kreposti*, 162–64; for Izborsk: construction in 1303, reconstructions of 1303, 1330, approx. 1395, and approx. 1450; *Pskovskie letopisi*, 1: 14, 17; Kostochkin, "Stroitel'naia," 126–43; for Oreshek: 1323, 1352, 1410; *NPL*, 100, 339; *PSRL*, 4, pt. 1: 110, 288, 410, and 16: 57, 159; for Tiverskii gorodok: approx. 1330s; Kirpichnikov, *Kamennye kreposti*, 145; for Ostrov: approx. 1350, rebuilt in 1380s; Rappoport, "Iz istorii," 190–91; and Kostochkin, "O datirovke," 59–65; for Korela: the tower in 1364; *PSRL*, 16: 90; Porkhov: construction, 1387; *NPL*, 381; reconstruction of it and Iamgorod, 1430, 1448; *NPL*, 415–16; *PSRL*, 16: 192; the Gdov fort: begun 1431; *Pskovskie letopisi*, 1: 39; the rebuilding of the fort at Staraia Ladoga in 1446; *PSRL*, 4: 123.

were constructed entirely of limestone blocks with a smooth exterior face, a more expensive material known as ashlar. In both building techniques and materials, these projects differed little from wall building elsewhere in Europe, where rough-cut blocks and rubble were used almost exclusively until 1300 and were common even after that. A. N. Kirpichnikov claimed that Novgorod and Pskov fortifications resembled those in Lithuania, Livonia, and Estonia, even to the facing of walls with alternating layers of granite boulders and limestone blocks. Perhaps so, but V. V. Kostochkin has pointed out important differences. Only the fort at Izborsk, for example, had a central tower resembling the keep or donjon of early European castles.³³ The problem requires further study.

In his study of monumental building in Suzdalia, Voronin put the circumference of the Moscow kremlin of Donskoi's time at 1,979 meters. It enclosed an area not much smaller than today's kremlin and probably had nine towers. Assuming that in thickness, height, and shape its walls were analogous to fortifications in the Novgorod land at that time (approximately two meters thick and eight meters high), and that it was built entirely of limestone, Voronin estimated that it required almost 54,000 cubic meters of stone, of which 14,371 cubic meters were ashlar blocks.³⁴ The calculations are crude and rest on assumptions about construction materials that cannot be tested. We are on firmer ground in using Voronin's formula to estimate the amount of stone used in building the Novgorod forts whose dimensions and building materials are known. The results allow a comparison of the size and relative cost of these constructions.

Of forts in the Novgorod land, Izborsk had the greatest circumference, about 510 meters, and Iamgorod the smallest, approximately 245 meters. Izborsk had numerous towers; Iamgorod two. Using Voronin's formula, I estimate that it required over 15,000 cubic meters of stone to build Izborsk, and about 6,637 to build Iamgorod.³⁵ When completed, the walls of the Novgorod kremlin had a circumference of about 1,465 meters, were three meters thick on the average, and boasted twelve towers. Calculating in the same manner, but increasing the result by one-third to adjust for the greater thickness of its walls, it comes out that builders used almost 60,000 cubic meters of stone!³⁶ Pskov had a smaller kremlin with outer dimensions of about 1,180 meters. But, being vulnerable to attack from the west, its walls were the highest and most massive in northern

³³ The stone wall of the Gdov fort continued alternating rows of boulders and stone to the top of its outer face. The forts at Korela and Tiverskii gorodok were crude affairs built of piled-up boulders. At Korela, only the lower levels were mortared. Tiverskii gorodok stood but two meters high and probably had a wooden parapet; Kirpichnikov, *Kamennye kreposti*, 35–67, 102–18, 114–15, 130–34, 146–49, 164, 168–78, 192–207, 226–55; Rappoport, "Iz istorii," 136–40, 149–64, 168–83, 187–91; Kostochkin, "Stroitel'naia," 126–33; Kostochkin, "Kreml'," 77–85; Kostochkin, "Voenno-oboronitel'noe," 455–56; Voronin, *Zodchestvo*, 2: 177–79, 228–30; Voronin, "Moskovskii kreml'," 62–65; Mongait, "Oboronitel'nye sooruzheniia," 60–91; Goldthwaite, *Building*, 221; Salzman, *Building*, 82–90, 149–54.

³⁴ Voronin, "Moskovskii kreml'," 63; Voronin, *Zodchestvo*, 2: 232. Kirpichnikov, *Kamennye kreposti*, 269, estimated the walls at Koper'e, Ladoga, and Porkhov before the age of artillery to be 2.2 to 3.3 meters thick and 7.5 to 8.8 meters high (with parapets).

³⁵ Kirpichnikov, *Kamennye kreposti*, 203; Rappoport, "Iz istorii," 169.

³⁶ From table, Voronin, *Zodchestvo*, 229; and diagram and text, Mongait, "Oboronitel'nye sooruzheniia," 59–61.

Rus'. Together with an often massive system of stone outer walls that were unique in northern Rus', it was by far the greatest construction project undertaken before 1463.³⁷ This magnitude of building tends to confirm the impression gained from a tally of projects, namely that, starting not long after 1300, northern Rus' was showing signs of steady economic growth.

Although less is known about construction costs in northern Rus' than in Western Europe, an appropriate place to begin is with a survey of limestone deposits. The cost of transporting stone, in person-days or in money, accounted for a large share of total construction costs. In England, Louis Salzman wrote, the cost of carting stone twelve miles amounted to the value of the stone itself. It cost less to move stone by water. Even so, hauling stone from a hundred miles away was an extraordinary event. Monetary costs of quarrying and moving stone in England probably differed from those in northern Rus', but the technology and the number of person-days of labor needed to do the job were much the same. For this reason, northern Rus' could not have been an exception to the rule that building in stone depended on its local availability.³⁸

Given the importance of the location of deposits for estimating construction costs, one would expect building stone to be the subject of a considerable literature. But, aside from Voronin's survey, now twenty-five years old, I am unaware of any study of Suzdalian quarries. Voronin mentioned limestone deposits on the Volga near Nizhnii Novgorod and in the basins of the Kliaz'ma and Oka rivers (the latter near Murom) that were close to building sites. He speculated that they were known before the Mongol invasion and that their nearness to the Bulgar khanate may account for the legend that Suzdalia had imported its limestone from distant quarries on the Kama River in the Bulgar land. Deposits at Staritsa and Zubtsov in western reaches of the Tver' principality near the Volga, Voronin believed, supplied builders in pre-Mongol Suzdalia and almost certainly were the source for subsequent construction in Tver'. Finally, he mentioned limestone quarries on the Moscow River, and particularly that at Miachkovo, fifty kilometers from Moscow, without which Grand Prince Dmitrii Donskoi could not have built his kremlin in so short a time.³⁹ We know even less about the quarries that might have supplied Pskov and Novgorod. All writers described Novgorod stone as a permeable gray limestone, presumably softer and less amenable to free cutting than that in Suzdalia, referring to it variously as "Il'men" or "Volkhov" stone without pinpointing quarry locations. Kostochkin wrote that Novgorod and Pskov forts were built partly of local gray limestone of the Devonian period. He also called attention to the use of a ferrous limestone found near Lake Il'men.⁴⁰ Lacking better information, I nevertheless can state

³⁷ Labutina, *Istoricheskaia topografiia*, 31–91; Aleshkovskii, "Nachal'nye etapy," 322–49; Kostochkin, "Voenno-oboronitel'nye," 420–21, who claimed that Pskov's walls were among the largest in Europe. But compare with Knoop and Jones, *Mediaeval Mason*, 2–3.

³⁸ Goldthwaite, *Building*, 212–37; Salzman, *Building*, 119–21.

³⁹ Voronin, *Zodchestvo*, 1: 106–07, 306–08, 325–29, 469, and 2: 116–17, 121, 230–31, 343.

⁴⁰ Gladenko, "Arkhitektura Novgoroda," 189; Karger, *Novgorod* (1st edn.), 269–70; Kostochkin, "Voenno-oboronitel'noe," 421–22, 429–31, 455; Rappoport, "Iz istorii," 168–79; Rybakov, *Remeslo*, 663–64.

that the use of limestone on such a scale in the Novgorod land could only have meant that quarries were numerous, close at hand, and accessible by water. It bears repeating, however, that scholars have yet to do tests that would match building stone with a particular quarry.

In Western Europe, patrons usually furnished the stone, and this probably held true in northern Rus'. Quarrying itself was a rural occupation and labor intensive. Only cutters were skilled workers. Unlike building, quarrying went on continually with only seasonal fluctuations. It produced rubble and cut blocks in about equal quantities.⁴¹ Hauling stone to building sites required a much greater work force, making transport the major cost variable in big projects. Although we have virtually no evidence about who transported stone to building sites in northern Rus' and how they were recruited, an adequate work force could only have been assembled by imposing labor obligations on urban and peasant communities. This was above all true of forts, many of which were built on short notice to meet a perceived threat.

It was under exactly such conditions that Dmitrii Donskoi in the fall of 1366 ordered that a stone kremlin be built in Moscow. The stone was brought to Moscow that winter, 1366–1367, over the ice (and not by water, as was common in Europe).⁴² To haul the 54,000 cubic meters of stone and rubble the fifty kilometers from the quarry to Moscow along the Moscow river, Prince Dmitrii had to assemble rapidly laborers, sledges or carts, and horses. Voronin estimated that it required 548,000 person-days to complete the job! This constituted over half of the estimated person-days needed for the entire project from the quarrying of the rock to the finishing of the battlements. If the winter season is defined as the 151 days from November 1 through March 31, it required at least 3,563 laborers, an equal number of sledges or sleighs, and probably twice that number of horses. Labor recruitment for hauling the stone for the Moscow kremlin was of a magnitude that had few counterparts. For comparison's sake, European rulers usually had to make do with less than a thousand conscripts in the "rush" construction of forts. Per capita labor costs for haulers in Suzdalia may not have been as high as those given by Salzman for England or anywhere else where workers were hired. But in medieval construction, as Richard Goldthwaite noted, labor was more valuable than money.⁴³ In the 1360s, Moscow's princes could command unusually large numbers of this commodity.

Similarities in techniques of stone construction in Europe (largely unchanged before 1800), dictated similar patterns of labor use in the thirteenth and

⁴¹ Salzman, *Building*, 119–28; Knoop and Jones, *Mediaeval Mason*, 8–13, 40–45; Voronin, *Zodchestvo*, 2: 147–48.

⁴² *PSRL*, 15, pt. 1 (2d edn.): 83–84; 18: 106. Moscow expected attacks from Tver' and Lithuania in the 1360s, not to mention the ever-present Mongol threat. In 1365, fire devastated Moscow, making the situation critical; Presniakov, *Formation*, 245–52; Cherepnin, *Obrazovanie*, 545–87; Voronin, *Zodchestvo*, 2: 175–76.

⁴³ Based on Voronin's assumption that they employed small peasant carts, *Zodchestvo*, 2: 228–34, with one driver per vehicle. Voronin thought one horse sufficed to pull a cart; it is more likely that two were needed, there being no evidence for large plow horses (or big wagons) in fourteenth-century Rus'. On methods, equipment, and investments of men and horses for hauling, see Knoop and Jones, *Mediaeval Mason*, 45–60; Fitchen, *Building*, 169–87; 117–24; Lynn White, Jr., *Medieval Technology and Social Change* (New York, 1964), 61–73; Goldthwaite, *Building*, 117–24.

fourteenth centuries. Because construction was based on restricted and periodic demand, it resisted efficient organization and, with few exceptions, remained local in managerial and supply mechanisms. Yet building was an occupation of greater scale than were other trades, requiring special methods for the hiring of laborers. Large projects demanded extraordinary methods of recruitment. Patrons generally took on local people at slack times in their normal occupations as unskilled laborers and paid them wages. Towns used a labor tax only for large civic projects; kings and other rulers pressed unskilled labor into service when they had to build a fort at short notice and for other exceptional projects. But rarely did they have the means or power to hold large numbers of workers for prolonged periods.⁴⁴

Situated in the northern extremes of Europe's agricultural zone, Novgorod or Pskov hardly had large reserves of unskilled labor. As in the West, they must have relied on a levy of citizens to build major projects, and their chronicles provide evidence that this was indeed so. In the entry for 1309, one reads that Mayor (*Posadnik*) Boris and the men of Pskov built a stone wall, or under 1323 that Prince Iurii Danilovich and the men of Novgorod built the fort of Oreshek. Where an entry states only that a prince, *posadnik*, or archbishop (as head of state), built a major project, we may assume that it also was a municipal endeavor. In 1430, Novgorod mobilized peasants to work on its walls. Suzdalia undoubtedly had a bigger pool of unskilled peasant labor. When the harvest was in, peasants had ample time during winter months for non-agricultural work. So, when Dmitrii Donskoi mobilized a virtual army of the unskilled on short notice to work on the kremlin, an adequate labor pool was ready and waiting. It is well to remember, however, that this was a one-time achievement that his successors were unable to duplicate before 1462. Also, there is no reason to equate peasant labor with servile obligation.⁴⁵

Nor can one easily make assumptions about whether unskilled workers on lesser projects worked for hire. For example, where in the Novgorod chronicle it is said that the men of Liubiantsa Street built the Church of St. George (1356), and those of Danslavl' Street the Church of St. Demetrius (1394), it is unclear whether the citizenry volunteered the labor or hired out the job. The latter could easily have been the case. As A. L. Shapiro has emphasized, the hiring of labor went on throughout the "feudal" era in northern Rus', particularly in construction and transport. Whichever way it was, the churches in Novgorod and in Suzdalia were small and required negligible labor inputs. Voronin estimated that it took 7,308 person-days over a year's time to quarry and haul the stone, and to build the small (139 square meters) Church of the Intercession on the Nerl' River (1166). This estimate of 7,308 person-days translates to twenty workers

⁴⁴ Fort building could strain the labor market: Goldthwaite, *Building*, 4–9, 117–24; Salzman, *Building*, 30–81; Knoop and Jones, *Mediaeval Mason*, 2–4.

⁴⁵ *Pskovskie letopisi*, 1: 14, and 2: 22; *NPL*, 339; *PSRL*, 16: 57, and further references to communal patronage, for example, the cooperation of Grand Prince Dmitrii (Alexandrovich) and Posadnik Mikhail (Mishinich) in building Kopor'e (1280), *NPL*, 323; *Pskovskie letopisi*, 1: 39; 2: 36, 43, 125; *PSRL*, 16: 192. See V. L. Iarin, *Ocherki kompleksnogo istochnikovedeniia: Srednevekovyi Novgorod* (Moscow, 1977), 230–37, on Novgorod's political leaders.

laboring each day for one year. If no more than half the work force did the actual building, as was estimated for fort construction, there would have been only ten builders, including masons. From a comparison of foundation sizes, it would have taken 6,230 person-days to build the largest church erected in Novgorod after 1238. As that job took up to three years, it would have needed but six to eight builders. By the same measure, only seven to nine workers would have sufficed to put in the 10,410 person-days over the up to four years that it would have taken to construct the largest church in Suzdalia.⁴⁶

Most Soviet writing on architecture and construction assumes that, for the most part, masons and other skilled construction laborers were in service to a lord in pre-Mongol Rus' and in the north after the conquest, even though evidence for the post-1238 era is virtually nonexistent. V. L. Ianin, it is true, demonstrated that a boiar oligarchy dominated Novgorod's politics and economy by 1300. From this, he concluded, townspeople who resided in and around boiar establishments, as well as country people, must have been dependents. Nevertheless, there is little direct evidence, nor is it likely, that townspeople came completely under boiar control, that all artisans lived on boiar property, or that those who did were necessarily dependents. No one can write with certainty about such matters, but if we conclude otherwise for the period after 1300, as Shapiro noted, it would set off northern Rus' sharply from the rest of Europe.⁴⁷

In Western and Central Europe, professional builders and masons were like modern workers in that they almost always hired themselves out for money. Whereas unskilled laborers, once paid, turned to other occupations, masons moved individually or with their apprentices from project to project, each time contracting for wages. Silicosis of the lungs or injury shortened their careers and lifespans, but, while they worked, they were well rewarded and notoriously difficult to organize and control. In Italy, where masons usually could work regularly in one locality, they formed well-organized guilds. In Northern Europe from England to the Baltic, masons had to move about to find regular work. As a result, theirs was the one important trade that did not have a stable guild organization. Even when they were impressed, and they often were, they were paid "a king's wage."⁴⁸ Could it have been so different in Novgorod and

⁴⁶ NPL, 364, 386, 397; PSRL, 4, pt. 1: 375; 30: 111; Voronin, *Zodchestvo*, 1: 325–26; A. L. Shapiro, *Russkoe krest'ianstvo pered zakreposticheniem* (Leningrad, 1987), 240–47.

⁴⁷ Rappoport, "Stroitel'nye arteli Drevnei Rusi i ikh zakazchiki," *Sovetskaiia Arkheologiia* (1985), no. 4, 80–89; P. A. Rappoport, "Zodchie i stroiteli drevnego Smolenska," *Drevniaia Rus' i Slaviane*, ed. T. V. Nikolaeva (Moscow, 1978), 403–06; Voronin, *Zodchestvo*, 1: 322–24, 329–36, 465–66, and 2: 120–21, 234, 364, offered evidence that builders were itinerant and for the most part dependent in pre-Mongol Rus', and assumed that they had shops. Voronin said little about the post-1238 status of builders but, by citing the Tver' chronicle that Donskoi mobilized workers to build the kremlin, and the treaty (approx. 1390) between Prince Vasilii Dmitrievich of Moscow and his cousin on their right to command "masters," he implied that they were dependents. Rybakov, *Remeslo*, 712 and following, wrote that Rus' masons, like counterparts elsewhere, did not have a shop organization, but he hedged as to their status. Compare divergent views on Novgorod society by Ianin, *Ocherki*, 230–37; Birnbaum, *Lord Novgorod*, 77–78. Also see Shapiro, *Russkoe krest'ianstvo*, 240–47.

⁴⁸ At large projects, masons organized lodges of fellow workers, which broke up when the work was done; Salzman, *Building*, 30–44; Goldthwaite, *Building*, 115–70, 242–350, esp. 126, 244–49; Murrav, *Building Troyes Cathedral*, 111; Knoop and Jones, *Mediaeval Mason*, 66–164; A. R. Bridbury, *Economic Growth: England in the Later Middle Ages* (London, 1962), 14–22; Rörig, *Medieval Town*, 160; Abel, *Agricultural Fluctuations*, 58–62.

Pskov after 1300, or even Suzdalia from the 1360s, where power and patronage were distributed widely enough to cause patrons to bid for masons and patrons rich enough to keep masons continuously busy?

Direct evidence about recruitment and pay of skilled workers relates almost entirely to Pskov, but what was customary there almost certainly held true for Novgorod. The evidence consists of twelve unequivocal references in the Pskov chronicle, all but one tabulated by B. A. Rybakov in his book on Rus' crafts, that skilled builders were hired for wages by the mid-1300s. Variants of the earliest entry state that in 1364 Pskov hired workers for five rubles (apiece) or 200 rubles to pull down the ruined walls of the Church of the Holy Trinity. In 1365, the chronicle recorded that Pskov hired "masters" to rebuild the church. The job took three years; for it, Pskov honored them and paid 400 rubles in wages (*mzdy*). Although this was a big project, the main structure of the church covering 876 square meters, fortifications cost far more: in 1424, after three-and-a-half years, 200 workers completed (rebuilding) part of the kremlin for which Pskov paid 1,000 rubles in wages and 200 rubles for bricks. Six entries specifically referred to masonry (and possibly brick) construction. Eight referred to builders as "masters," and two provided names: a master named Kirill built a church in 1373, and in 1420 "the Pskovites hired (*naiasha*) the masters Fedor and his associates (*drouzhinu*) to provide the Church of the Holy Trinity with new roof panels of lead. The Pskovites could not find masters who knew how to cast leaden panels in Pskov or Novgorod, so they sent to the Germans in Iur'ev (Dorpat), but the accursed ones did not send a master. Finally a master arrived from Metropolitan Photios in Moscow, and master Fedor repaired the Holy Trinity and returned to Moscow."⁴⁹

For Suzdalia, one must work more from inference than evidence in discussing the status of skilled builders. Compared with Novgorod, its patrons were fewer in number and in sum less wealthy. Because most commissions were equally small in size but fewer in number and farther between, it is likely that masons, also fewer in number, had to move about to keep working. Voronin maintained that since Dmitrii Donskoi's time a construction team existed in Moscow and that it worked continuously, if not there, then in apanage centers. But, by the 1390s, Voronin's hypothetical team and possibly other builders could obtain orders from boiars, bishops, and monasteries as well as from Moscow's prince, the metropolitan and apanage princes. Voronin also was convinced that Tver' by then had a small shop working regularly under the patronage of its ruling house. Thus it appears that Suzdalia had a patronage base rich enough to create a demand and one diversified enough to compete for builders. The few bits of

⁴⁹ Rybakov, *Remeslo*, 708, also listed seven post-1462 entries and attempted, 709–11, to compute wages in real terms; *Pskovskie letopisi*, 1:23, 26, 34–35, 39, 54–55; 2: 27–28, 36, 39, 43, 49, 103, 109, 125, 131, 143–44, 152; A. A. Tits, "Obmernye i proeknye chertezhi XVII v. Troitskogo sobora v Pskove," *Srednevekovaiia Rus'*, D. S. Likhachev, et al., eds. (Moscow, 1976), 336–41; Aleshkovskii, "Nachal'nye etapy," 333–35, on dates and location of work on Pskov's walls; and Birnbaum, *Lord Novgorod*, 44–100; V. L. Ianin, *Novgorodskie posadniki* (Moscow, 1962); Ianin, *Ocherki*; Ianin, *Novgorodskaiia*; V. N. Bernadskii, *Novgorod i novgorodskaiia zemlia v XV veke* (Moscow, 1961), 52–266; B. B. Kafengauz, *Drevnii Pskov* (Moscow, 1969); Iu. G. Alekseev, *Pskovskaia Sudnaia gramota i ee vremia* (Leningrad, 1980), esp. 14–19, 27–44, on the socioeconomic profile of Novgorod and Pskov.

hard evidence that exist support this view. A professional builder, Vasilii Ermolin, was hired to oversee repair of the Kremlin in 1462. There are also two references to work for hire that were not in construction: in 1346, a master named Borisko, probably an Italian, cast bells in Moscow, and a monk Lazar the Serb received "over 150 rubles ["sta bole polutorasta rublev"]" in 1404 to make a clock ("chasy postavi a chiudni i s lunoiu") in the grand princely palace.⁵⁰

To sum up, the evidence, even if it is too sketchy to make sharp quantitative comparisons of wealth, does suggest that the way buildings were built and labor recruited in northern Rus' differed little from how such things were done in the rest of Europe. Also, it often supports and never runs counter to statistical indications that northern Rus' experienced a disastrous economic depression for ninety years after the Mongol invasion, followed by an equally dramatic upturn that continued almost unabated to 1462. It remains to inquire what building records reveal about the relative wealth of principalities, how wealth was held in each, and how this translated into political power in northern Rus'.

LOOKING AGAIN AT FIGURE 4, which shows monumental construction by principality, 1238–1462, we must wonder at Novgorod's wealth. The economic upturn came there first and most strongly; for the century 1238–1337, twenty-nine of forty (or 73 percent) of all projects, including all forts and walls, were built in the Novgorod land. Even after the recovery reached Suzdalia and the economy of northern Rus' "took off," Novgorod's record easily surpassed that of other principalities. It built 147 (or 53 percent) of 280 projects recorded between 1338 and 1462. If one adds the sixty-eight projects built in the Pskov principality after independence from Novgorod in 1347, the combined total is an overwhelming 77 percent. It includes every stone fort and wall built in northern Rus' except the Moscow kremlin and the partially stone kremlin in Nizhnii Novgorod. Novgorod even finished six churches in the plague year 1417.⁵¹

Patrons of between seventy-seven and eighty-five of 147 "private" projects in the Novgorod land can be identified. Novgorod's archbishops (not as head of state) were most prolific, building forty-four to forty-six structures, one co-financed by the Grand Prince of Vladimir. Boiars built from seventeen to twenty, commoners nine to eleven, and together they built two, totaling 38 percent of known patronage. It is likely that these groups also built most of the private projects for which no patron of record is listed, if only because forty-two buildings were on sites that suggest lay patronage. Abbots built the remaining six, four dating to 1310 or earlier.⁵² Boiars and prominent citizens thus had

⁵⁰ The Nikon chronicle called Master Boris "a Roman"; Voronin, *Zodchestvo*, 2: 364–66, 445; *PSRL*, 10: 217; 18: 96; 23: 157; 25: 232–33.

⁵¹ *NPL*, 407. Pskov built only five projects before independence.

⁵² Archbishops and boiars eclipsed princes as patrons from the 1130s; Rappoport, "Stroitel'nye," 86; Bulkin, "Arkhitekturno-stroitel'naia," 218. Episcopal patronage: *NPL*, 327, 328, 331, 334–35, 339, 343, 346–48, 354, 362, 367, 369, 393, 394, 400–01, 403, 407, 415–16, 418–25; *TL*, 382; *PSRL*, 4, pt. 1: 280, 290–91, 384, 389; 30: 107, 111. Boiar construction: *TL*, 382, 385; *NPL*, 332–33, 364–65, 379–80, 383, 385, 387, 388, 393, 397, 400, 404, 414, 417; *PSRL*, 4, pt. 1: 287, 295, 305, 351,

considerable wealth, as one might expect in a mercantile society. They used it to build family chapels (at least nineteen) and parish churches, and to purchase country estates. These estates often were endowments for “founder” (*ktitorskie*) monasteries, for which they built at least eleven stone churches in and outside of town and over which the patron’s family retained control.⁵³ As far as is known, not one boiar built a stone palace before 1463.

Ianin and the historian of the Novgorod church, A. S. Khoroshev, make a solid case that parish and private churches were substantially free of episcopal control and that “founder” monasteries were totally free of the archbishop. Patrons thus protected their investment, not only from the archbishop but from division by family inheritance. Novgorod’s patricians no doubt had other motives. Parish churches were at once statements of self-esteem, civic virtue, and power. Goldthwaite’s description of the similar building in contemporary Florence as a “happy convergence of a different kind of public duty and purely private interests” is equally appropriate for Novgorod. “Founder” monasteries were refuges. There boiars and their relatives might be tonsured and find peace at the end of their days. There they could ensure that prayers were said for the souls of their ancestors and, when they died, for their own. Everywhere in Europe, plague caused elites to build personal or family memorials.⁵⁴ Novgorod’s patriciate must have done the same. The churches were small, and one can hardly doubt that this was more by choice, because of the purposes for which they were built, than the result of labor shortages caused by plague.

Clearly, Novgorod played a key role in the political life of northern Rus’. The right to collect tribute in wealthy Novgorod was one of the most important benefits of being grand prince, especially so from 1238 to at least 1320, when Suzdalia’s economy lay in ruins. We can easily appreciate why Grand Prince Aleksandr Nevskii and his successors considered it more important to control and defend Novgorod than to war for possessions in Suzdalia. Yet, if wealth alone sufficed, Novgorod would have become the dominant power in northern Rus’. To the causes that historians have given for Novgorod’s failure to do so—its geographic vulnerability, internal instability, alleged inability to feed

382–83; 11: 238; 16: 90, 193; 30: 108, 112; Ianin, *Ocherki*, 152–59. Commoner building: *TL*, 382; *NPL*, 364, 368–69, 386, 397, 425; *PSRL*, 4, pt. 1: 290, 292, 375; 30: 111, 114; Karger, *Novgorod* (1st edn.), 140–41. Joint projects: *TL*, 394; *NPL*, 372; V. N. Lazarev, *Iskusstvo Novgoroda* (Moscow, 1947), 102–04. By abbots: *NPL*, 328, 333, 402, 412; *Pskovskie letopisi*, 1: 14; *PSRL*, 30: 99. Boiars may have been patrons to some churches said to have been built by residents of a street and ascribed to commoners. Assumptions that unidentified patrons were lay people also follows from knowledge that clerics wrote the chronicles and had better knowledge of church patronage and better reason to record it.

⁵³ V. L. Ianin, “‘Semisobornaia rospis’ Novgoroda,” *Srednevekovaiia Rus’*, D. S. Likhachev, et al., eds. (Moscow, 1976), 108–17; V. L. Ianin, “Iz istorii vysshikh gosudarstvennykh dolzhnostei v Novgorode,” *Problemy obshchestvenno-politicheskoi istorii Rossii i slavianskikh stran. Sb. statei k 70-letiiu Akademika M. N. Tikhomirova* (Moscow, 1963), 118–27; A. S. Khoroshev, *Tserkov’ v sotsial’no-politicheskoi sisteme novgorodskoi feodal’noi respubliki* (Moscow, 1980), 159–77, and list of monasteries and their lands, appendix 2, 203–13.

⁵⁴ Ianin, “‘Semisobornaia rospis’,” 108–17; Ianin, “Iz istorii,” 118–27; Khoroshev, *Tserkov’*, 156–77. Consult Nancy Shields Kollmann, *Kinship and Politics: The Making of the Muscovite Political System, 1345–1547* (Stanford, Calif., 1987), on kinship in medieval Russian culture; and Goldthwaite, *Building*, 12, on post-plague western building trends.

itself, and demographic weakness—I would add one more: as Novgorod grew rich, its leaders spent considerable amounts on private construction and land acquisition. Construction brought prosperity to the building trades and to those who controlled building materials, but it neither generated economic growth nor contributed to the town's security. In their spending priorities, Novgorod's elite was not so different from contemporary upper classes in Flanders and Florence.⁵⁵ In fairness, Novgorod's leaders spent considerable sums on the city's kremlin and on forts that protected its trade routes to the West. And, between 1438 and 1462, when western artillery threatened the city's forts, Novgorodians stopped private building almost entirely in order to reinforce them. Why they did not see the need to better fortify their eastern frontier awaits a satisfactory explanation.

In Suzdalia, building records indicate that Moscow and, to a lesser extent Tver', began to generate increasing wealth in the fourteenth century, although per capita income there must have been far less than in the Novgorod land. Also, as might be expected in largely agricultural Suzdalia, wealth was more narrowly held and patrons fewer in number. In fact, senior princes and metropolitans had a hand in 68 percent of the projects for which patrons can be identified. Monastic patrons were a distant second, participating in 15 percent, a third of these being in partnership with princes and metropolitans. Apanage princes were still further down the list, and even this measure does not adequately indicate their relative economic weakness. Patronage records also indicate that senior princely families built only in their cities of residence with very few exceptions. Although Vladimir was the capital of northern Rus' until Donskoi's reign (1360–1389), the only construction in Vladimir after 1237 was a tiny chapel attached in 1300 to the Church of the Dormition, probably by Metropolitan Maxim.⁵⁶

Between 1285 and 1290, Prince Mikhail Iaroslavich, his mother Kseniia Georg'evna, and Bishop Semen built the episcopal Church of the Transfiguration in Tver', the first new stone structure built in Suzdalia after the conquest. Tver' continued to build masonry structures until 1462 in every quarter-century but one, 1363–1387, in which it fell on hard times after siding with Lithuania in a losing struggle against Dmitrii Donskoi. One can hardly doubt that the economy of Tver', and its record of construction, would have been more buoyant had Mikhail Iaroslavich (d. 1318) kept the grand princely crown of Vladimir for his line. In the fifteenth century, the rate of building picked up, a likely sign of better times. The princes and bishops of Tver' surely controlled most of this wealth. Of fourteen buildings built after 1387, senior princes paid for nine and possibly a tenth; the bishop of Tver' built three, and together they built another. None was built by an apanage prince, a fact that strengthens the

⁵⁵ Nicholas, *Towns and Countryside*, 267–329; Herlihy, "Distribution," 155–57; Goldthwaite, *Building*, 29–30, 50–51; Goldthwaite, *Private Wealth*, 78–85 and following; Martines, *Power and Imagination*, 171–72.

⁵⁶ Its foundation was 42 sq. meters. *PSRL*, 30: 101; Voronin, *Zodchestvo*, 1: 355–56; Sakharov, *Goroda*, 48–49.

Distribution of Patronage in Suzdalia, 1238–1462

<i>Patron</i>	<i>Known</i>	<i>Probable</i>
Princes*	25	5
with Metropolitan	1	0
with Monasteries	1	2
with Bishops	2	0
with Boiar	1	0
with Boiar & Metropolitan	1	0
Total, Princes	31	7
Metropolitans	6	4
with Prince	1	0
with Prince, Boiar	1	0
with Monasteries	2	0
Total, Metropolitans	8 (10)	4
Bishops	7	1
with Princes	2	0
Monasteries	2	3
with Princes	1	2
with Metropolitans	2	0
with Apanage Prince	1	0
Apanage Princes	3	2
with Monasteries	1	0
Boiars	4	0
with Prince	1	0
with Prince, Metropolitan	1	0
Total, Others	17 (25)	6 (8)
Total, Unknown	3	0

* Includes princes of Moscow, Tver', and Nizhnii Novgorod.

case of those who have argued that Tver' was not seriously weakened by apanage division.⁵⁷

Nizhnii Novgorod experienced a spurt of monumental building after 1341, when the Mongols made its prince, Konstantin Vasil'evich of Suzdal', grand prince and a rival of Moscow. As in Tver', construction was almost entirely the

⁵⁷ Patronage records: *TL*, 343, 345, 449; *PSRL*, 4, pt. 1: 246; 6: 132; 11: 156, 202; 15: 470, 488, 490–91, 494–95; 15, pt. 1 (2d edn.): 165–66, 186; 25: 233; N. P. Likhachev, *Inoka Fomy Slovo pokhval'noe o blagovernom Kniaze Borise Aleksandroviche* (St. Petersburg, 1898); Langer, "Russian Medieval Town," 88–93; Voronin, *Zodchestvo*, 2: 137–43, 373–414. Dmitrii Donskoi and Algirdas twice fought across Tver' territory; Dmitrii besieged Tver' and forced its surrender in 1375; Cherepnin, *Obrazovanie*, 557–82. Presniakov, *Formation*, 172–87, maintained that apanage divisiveness weakened Tver'; V. A. Kuchkin stated that it was insignificant, *Formirovanie gosudarstvennoi territorii severo-vostochnoi Rusi v X–XIV vv.* (Moscow, 1984), 167–98. Also see E. Klug, "Das Fürstentum Tver' (1247–1485)," *Forschungen zur osteuropäischen Geschichte*, 37 (1985): esp. 123–25, 148–57, 209–16, 230–32.

result of princely generosity. Konstantin made Nizhnii Novgorod an episcopal see and rebuilt the thirteenth-century Cathedral of Our Savior in 1350–52. The Mongols ceased to favor Konstantin after 1353, causing him and his sons to come under Moscow's domination. But Nizhnii Novgorod continued to build until the mid-1370s, its prosperity levered by commerce and perhaps subsidies from Moscow owing to its strategic frontier position at the confluence of the Oka and Volga rivers. Metropolitan Aleksei built a stone church there; Boris and Andrei Konstantinovich built two. In 1365, Boris began to wall the kremlin in stone, and his brother Dmitrii later resumed construction. But, before it was finished, a Mongol civil war spilled into Suzdalia. Mongol armies sacked and burned Nizhnii in 1376 and 1378, bringing its prosperity and building boom to an abrupt end.⁵⁸

In 1326, Grand Prince Ivan Kalita and Metropolitan Petr built the Cathedral of the Dormition in the Moscow kremlin. Although it was small, it was Moscow's first stone building and one of its most important, bringing the metropolitanate into joint tenancy and often political cooperation with Moscow's princes, and creating the basis for Moscow's earliest ideological claims to rule northern Rus'.⁵⁹ Thenceforth, Moscow's record of masonry construction in each time period was greater than the combined total for all other principalities in Suzdalia. In the last quarter-century, 1438–1462, it was the only principality in northern Rus' to show an increase.

Patronage records indicate that the principal residents of the Moscow kremlin, the princes of Moscow or their spouses, alone or with others, built from fifteen to nineteen stone structures, and the metropolitans of Rus' (after Petr) funded or co-sponsored construction of seven and possibly eleven. Including their joint patronage of the Cathedral of the Dormition, they accounted for up to 63 percent of the forty-nine masonry buildings in the Moscow and, later, Moscow-Vladimir principality. Moscow's princes also helped Novgorod build its fort at

⁵⁸ TL, 392, 394; PSRL, 8: 17; 10: 230; 23: 116, 164; 25: 238, 263; 15, pt. 1 (2d edn.): 60, 74, 100; *Nizhegorodskii letopisets*, 12–17; Voronin, *Zodchestva*, 2: 43–54, 208–16. Nizhnii Novgorod was not listed as a town “of stone” in the addendum to the “Commission” ms. of the Novgorod chronicle entitled *a se imena gradom ruskym, dalnim i blizhnim*, NPL, 477. Tatars sacked Nizhnii Novgorod again in 1409 and 1445; Presniakov, *Formation*, 215–25; Fennell, *Emergence*, 214–19; Kuchkin, *Formirovanie*, 199–263; Cherepnin, *Obrazovanie*, 582–96, 663–73; Langer, “Russian Medieval Town,” 108–19.

⁵⁹ V. A. Kuchkin, “Skazanie o smerti metropolita Petra,” *Trudy otdela drevnerusskoi literatury*, 18 (1962): 59–79. Metropolitan Petr's successors lived in the kremlin (finally in the Miracle Monastery built by Metropolitan Aleksei), held services in its Church of the Dormition, and were buried there. Although they quarreled with Moscow's princes, their interests more often overlapped, causing one to support or act in consort with the other. The subject has a large literature: E. Golubinskii, *Istoriia russkoi tserkvi*, 2 vols. (St. Petersburg, 1900–17), 2, pt. 1: 105–15, 133–44, 146–53, 172–87, 190–95, 198–216, 226–62, 299–345, 361–89, 484–514; John Meyendorff, *Byzantium and the Rise of Russia* (Cambridge, Mass. 1981); Presniakov, *Formation*, 113–15, 238–45, 252–61, 292–312, 382–85; Fennell, *Emergence*, 68–73, 124–34, 191–92; I. B. Grekov, *Vostochnaia Evropa i upadok Zolotoi Ord* (Moscow, 1975), 105–27 and following; G. M. Prokhorov, *Povest' o Mitiaie* (Leningrad, 1978); Gustave Alef, “Moscow and the Council of Florence,” *American Slavic and East European Review*, 20 (1961): 389–401; N. S. Borisov, “Moskovskie kniazia i russkie metropolity XIV veka,” *Voprosy istorii* (1986), no. 8, 30–43; N. S. Borisov, *Russkaia tserkov' v politicheskoi bor'be XIV–XV vekov* (Moscow, 1986). On the political significance of ecclesiastical relations with Lithuania, see also Kazimierz Chodynicki, *Kościół Prawosławny a Rzeczpospolita Polska (1370–1632)* (Warsaw, 1934); Horst Jablonowski, *Westrussland zwischen Wilna und Moskau* (Leiden, 1961), 49–55, 74–95; and letter of Metropolitan Iona, *Russkaia istoricheskaia biblioteka*, 6 (2d edn., 1908), no. 81, cols. 619–26.

Oreshek; together with its archbishop, they built a church in the prince's residence in Novgorod (1342–1343); and they assisted Pskov in wall construction (approximately 1380). The metropolitan built a church in Nizhnii Novgorod, noted above. Although the sources of income of the metropolitanate and of Moscow's princes are generally known, there is little specific information, even in the fifteenth century, about amounts. But, for most of the period after 1237, the favor of the Mongol khans was a major reason for the prosperity evident in patronage records of these dignitaries, above all for Moscow's princes, given that they held no unusual geographical advantage in Suzdalia or special resources. Success in winning, holding, and using the grand princely title was crucial if they were to prosper.⁶⁰

Names of lay patrons other than Moscow's princes in the construction records provide a pattern of wealth overwhelmingly tied to the kremlin. In 1340, Boiar Protasii was co-patron with Grand Prince Ivan Kalita of a monastic church in Moscow. Protasii was founder of the Vel'iaminov-Vorontsov boiar clan. His line was said to have come to Moscow with its first prince, Daniil, and probably resided in the kremlin. From about 1368, members of the Khovrin family built and rebuilt a stone church in their family palace in the Moscow kremlin and were sponsors, in one case with the grand prince, of two buildings and a wall of the Simonov Monastery in Moscow. A descendant of a Greek merchant family of the Genoese town of Sudak in the Crimea, the first Khovrin (originally Khovra), Stefan, came to Moscow around 1350 and lived in the kremlin. His grandson attained boiar rank. The Simonov Monastery was notorious for political ties with Moscow's princes as well as for its wealth and influence in the church. Residents of the kremlin may also have been the unknown patrons of a chapel in the Simonov Monastery and of the Church of St. George in a monastery of the same name on the Neglinnaia River in Moscow.⁶¹

There exist but four certain and three probable records of patronage by apanage princes. In the late 1370s, apanage Prince Vladimir Andreevich built a monastic church and refectory in his domain of Serpukhov (he also had a

⁶⁰ Princely patronage: *PSRL*, 5: 256, 274; 6: 130, 235–39; 18: 129, 151; 23: 102, 114, 133, 157; 25: 277; 1 (2d edn.), 531; *TL*, 359, 360–61, 384, 419, 440, 443–44; Voronin, *Zodchestvo*, 2: 152–53, 157–64, 175–79, 185–86, 194–203, 245–64, 266; V. A. Kuchkin, "K istorii kamennogo stroitel'stva v moskovskom kremle v XV v.," *Srednevekovaiia Rus'*, D. S. Likhachev, et al., eds. (Moscow, 1976), 293–97; Veselovskii, *Issledovaniia*, 211–12, 444. *TL*, 371; Voronin, *Zodchestvo*, 2: 159–60, 189–94, 205, mention buildings they probably funded. Princely endowments in other lands: *NPL*, 339, 354; *Pskovskie letopisi*, 1: 23; *PSRL*, 16: 57; 30: 107. Patronage of metropolitans: *TL*, 381, 406; *PSRL*, 8: 28, 30; 23: 113; 30: 125; Voronin, *Zodchestvo*, 2: 181–82, 206; and probable instances: *PSRL*, 25: 271, 276; 21: 315–16; Voronin, *Zodchestvo*, 1: 355–56, 2: 265–66, 325–37. Patronage by prince and metropolitan in Nizhnii Novgorod: *TL*, 358; *PSRL*, 23: 116; 25: 167–68; Voronin, *Zodchestvo*, 2: 152–57, 215–16. On church administration and wealth, see Golubinskii, *Istoriia*, 2, pt. 2; Veselovskii, *Feodal'noe zemlevladienie*, 329–455. To my knowledge, Mark Zlotnik, "Muscovite Fiscal Policy," *Russian History*, 6 (1979): 343–58, is the only study of princely revenues. It is descriptive and focuses on post-1462 Muscovy.

⁶¹ Records: *PSRL*, 6: 237; 18: 151, 205; 22, pt. 1: 510; 23: 154; 25: 275; Voronin, *Zodchestvo*, 2: 163–64, 185–86, 266; Veselovskii, *Issledovaniia*, 444–45. On Khovrins and Vel'iaminovs: Veselovskii, *Issledovaniia*, 211–29, 442–49; Gustave Alef, "Diaspora Greeks in Moscow," *Byzantine Studies*, 6 (1975): 26–34; and Kollmann, *Kinship*, 49, 209–10, 237–38 and following, who, unlike the others, called Stefan's son Vladimir. Also see Raba, *Moscow Kremlin*, 7–10; I. U. Budovnits, *Monastyri na Rusi i bor'ba s nimi krest'ian v XIV–XVI vv.* (Moscow, 1966), 69–70, 82–84; Ivina, *Krupnaia votchina*, 34–83.

residence in the Moscow kremlin). He was a wealthy man, richer perhaps than the prince of Tver', though, judging by construction records, far less wealthy than Moscow's rulers. But for one minor exception, Vladimir and his descendants also strongly supported the senior branch of his line in Moscow. The same held for Prince Andrei Dmitrievich, who very likely was patron of two churches built in his apanage of Mozhaisk in the 1390s. Only his brother Iurii, who built or co-sponsored two churches and probably another (two in Zvenigorod, one at the Trinity-St. Sergei Monastery), and Iurii's sons challenged the hegemony of Grand Prince Vasilii Vasil'evich (1425–1462) of Moscow. The relative economic insignificance of these records is in part the result of the disproportionately heavy share of the Mongol tribute that Moscow's princes placed on apanage princes. It also helps us to understand why Iurii's line was no match for Vasilii and his allies in the dynastic civil war of the 1420s to 1440s. Bishops or abbots built six other structures in Suzdalia.⁶² There is no evidence that Moscow merchants ever built stone churches (or palaces), nor do sources allow us to speculate as to why they differed in this with their Novgorodian counterparts.

Among recipients of patronage in Moscow's domains, monasteries led the way, being the object of construction grants in twenty-five of the forty-nine recorded cases. Non-monastic patrons built ten to twelve of these and in six other cases provided a share of the funds. Political wisdom and, as in Novgorod, self-esteem and religious motives combined to account for most of the donations. Only eight of these projects were built in or by monasteries of the "common-life" (*obshchezhitnye*), four being in the Simonov Monastery and completed in 1404 and after. The Trinity-St. Sergei Monastery, the first monastery of this sort in Suzdalia and the progenitor of most others in northern Rus', became rich enough, and forgetful enough of the founder's concern for poverty, to build two and perhaps three masonry buildings, beginning about 1405. Apparently, these were the only cases in which a monastic community had both means and independence to act on its own.⁶³

JUDGING BY CONSTRUCTION RECORDS, Moscow from the 1330s became much richer than Tver' and other principalities in Suzdalia. However, it was far less

⁶² Records: *TL*, 396–97; *Pamiatniki arkhitektury*, 2: 215, 220–22; Voronin, *Zodchestvo*, 2: 207, 267–306, 311–20; about apanage princes and the dynastic war, see Presniakov, *Formation*, 147–64, 283, 312–39; L. V. Cherepnin, *Russkie feodal'nye arkhivy XIV–XV vekov*, 2 vols. (Moscow, 1948–51), 1: 31–45, 63–80, 100–50; Cherepnin, *Obrazovanie*, 715–810; Gustave Alef, "A History of the Muscovite Civil War: The Reign of Vasilii II (1425–1462)" (Ph.D. dissertation, Princeton University, 1956); Nancy Shields Kollmann, "Kinship and Politics: The Origin and Evolution of the Muscovite Boiar Elite in the Fifteenth Century," (Ph.D. dissertation, Harvard University, 1980), 273–342, has a more satisfactory discussion of the dynastic war than Kollmann, *Kinship*, 133–34, 153–57; V. A. Kuchkin, "Spodvizhnik Dmitrii Donskogo," *Voprosy istorii* (1979), no. 8, 104–16; A. L. Khoroshkevich, "K vzaimootnosheniiam kniaziei moskovskogo doma vo vtoroi polovine XIV–nachale XV veka," *Voprosy istorii* (1980), no. 6, 170–74; Roublev, "Le tribut," 487–530 on the burden of Mongol tribute. Records for bishops and abbots: *PSRL*, 23: 144, 25: 277, 1 (2d edn.): 539; Voronin, *Zodchestvo*, 2: 203–05, 266, 307–10.

⁶³ Building records: *PSRL*, 1 (2d edn.), 531; 5: 256, 274; 8: 28, 31, 244; 12: 113, 198; 18: 151; 22, pt. 1: 510; 23: 113; 25: 233, 275, 277; 30: 125; *TL*, 360–61, 381, 396–97, 406; Voronin, *Zodchestvo*, 2: 157–64, 181–82, 185–86, 189–94, 205–07, 225–27, 264–66, 279–89, 299–306, 311–24; Veselovskii, *Issledovaniia*, 211–12, 444–45. Two patrons remain unidentified. On the importance of Sergei's movement in the economic revival of northern Rus': Langer, "Plague"; Budovniits, *Monastyri*, 112–258; Ivina, *Krupnaia votchina*, 34–84; Goehrke, *Die Wüstungen*, 63–65.

affluent than Novgorod or Pskov, although it may have been gaining on them shortly before 1462. If there were accurate population estimates for Novgorod, Pskov, and Moscow, they would certainly show that disparities in per capita wealth between Novgorod and Pskov as against Moscow, calculated as a ratio of building units to population, were even greater. In Suzdalia, wealth was narrowly held, being in the strongboxes of princes or, as with Moscow, also in the coffers of kremlin adherents and allies. Moscow's rulers were also the only princes to build in other principalities. Finally, it should be stressed that Dmitrii Donskoi of Moscow in 1367–1368 invested in a kremlin of stone. Although its walls were thinner than those of Novgorod and Pskov that came to be built to withstand western artillery, and although some of its walls crumbled in the fire of 1445 and were partly rebuilt of wood, it was the only stone kremlin in Suzdalia. With its construction, Dmitrii and his successors gained an important military advantage over their rivals. In 1368 and 1370, the kremlin withstood three-day and eight-day sieges by armies of Prince Algirdas of Lithuania. Khan Tokhtamysh, it is true, took it by force in 1382, scaling its walls with ladders—the Ermolin chronicle reports that “the kremlin then was still low”—to loot and burn its palaces and churches.⁶⁴ But the single-mindedness with which its princes concentrated wealth and used it for military advantage must count, along with their diplomatic skill, unusually close-knit political institutions, and luck in producing just enough male progeny, among the principal reasons for Moscow's success in winning political sway over northern Rus'.

⁶⁴ By trickery, Tokhtamysh lured Moscow's commander and notables from the kremlin and killed them. The earliest accounts of the attack clearly state that he then took the kremlin by siege using ladders to scale its walls; *TL*, 384, 388, 391, 423; *PSRL*, 15, pt. 1 (2d edn.), 143–44; 23: 114, 127–29, 157; Voronin, *Zodchestvo*, 2: 175–81; Voronin, “Moskovskii kremli,” 52–66; Salmina, “Povest',” 134–51; Cherepnin, *Obrazovanie*, 642; Raba, *Moscow Kremlin*, 8–10. Later, embellished accounts said that by deceit Tokhtamysh's troops also gained entry through the kremlin gate: *PSRL*, 4, pt. 1: 332–33, accepted by George Vernadsky, *The Mongols and Russia* (New Haven, Conn., 1953), 266. I am unclear why Raba considered the citadel at Tver', built entirely of wood, to be stronger than the Moscow kremlin in the mid-fifteenth century.

Review Article
The Return of Medieval Politics

CHARLES T. WOOD

John W. Baldwin, **The Government of Philip Augustus: Foundations of French Royal Power in the Middle Ages** (Berkeley and Los Angeles: University of California Press, 1986).

Chris Given-Wilson, **The Royal Household and the King's Affinity: Service, Politics and Finance in England, 1360–1413** (New Haven, Conn.: Yale University Press, 1986).

Judith A. Green, **The Government of England under Henry I** (Cambridge: Cambridge University Press, 1986).

William C. Jordan, **Louis IX and the Challenge of the Crusade** (Princeton, N.J.: Princeton University Press, 1979).

Andrew W. Lewis, **Royal Succession in Capetian France: Studies on Familial Order and the State** (Cambridge, Mass.: Harvard University Press, 1981).

Joseph R. Strayer, **The Reign of Philip the Fair** (Princeton, N.J.: Princeton University Press, 1980).

W. L. Warren, **The Governance of Norman and Angevin England 1086–1272** (Stanford, Calif.: Stanford University Press, 1987).

IN 1921, DANA CARLETON MUNRO finished the manuscript of *The Middle Ages 395–1272*, one of the first genuinely successful medieval textbooks to be written by an American. Upon publication, it gained immediate acceptance, and it long remained a standard for anyone seeking more than a brief overview of the period. In fact, only in 1937 did it begin to encounter a serious rival, James Westfall Thompson and Edgar Nathaniel Johnson's *An Introduction to Medieval Europe 300–1500*.

Although Munro made every effort to cover medieval civilization as a whole—including a chapter on “Monasticism,” for example, as well as others on “The Peasants,” “The Universities,” and “Heresy and the Friars”—the book's major themes demonstrate the extent to which its author saw “true history” as being institutional and political. Moreover, because the emergence of the nation state provided the backbone of Munro's story, he tended to downgrade the accomplishments of anyone who had pursued a different vision, notably of those German rulers who had sought to realize their imperial dreams in Italy:

The connection with Italy and Rome brought to the Germans a higher civilization than they had known before. It also brought about a political relation with the Italian peninsula which was destined to influence the course of history for many generations. It prevented either Germany or Italy from becoming a strong and united nation, as Germany exhausted her men and resources in the attempt to maintain the imperial power in Italy, and Italy was unable to develop a nationality of its own, because of its subjection to the foreign emperor.¹

Doubtless this passage continues to echo anti-German sentiments left over from World War I, but at the same time it conveys something more: Munro's own belief in the inevitable—and lasting—benefits of national self-determination. And, at a deeper level, it also reveals the extent to which writers of textbooks really do depend, as is often suspected, on the findings of earlier scholarship, in this case, that of those nineteenth-century historians whose chief purpose was to tell of their nation's successes and of the unique national character that had made them possible. Here, of course, William Stubbs provides a telling example, for in the opening pages of his *Constitutional History of England in Its Origins and Development* he, like most of his Continental contemporaries, stressed themes and a view of history that seem the source of Munro's approach forty years later:

The History of Institutions . . . has a deep value and an abiding interest to those who have the courage to work on it . . . For the roots of the present lie deep in the past, and nothing in the past is dead to the man who would learn how the present comes to be what it is . . . The growth of the English Constitution, which is the subject of this book, is the resultant of three forces, whose reciprocal influences are constant, subtle, and intricate. These are the national character, the external history, and the institutions of the people. The direct analysis of the combination forms no portion of our task, for . . . the national character has been formed by the course of the national history quite as certainly as the national history has been developed by the working of the national character; and the institutions in which the newly conscious nation is clothed may be either the work of the constructive genius of the growing race, or simply the result of the discipline of its external history. It would then be very rash and unsafe to attempt to assign positively to any one of the three forces the causation of any particular movement or the origin of any particular measure.²

Dated as Stubbs's assumptions may now appear, they were widely shared and in somewhat altered form had the power, as late as 1941, to provide unexpected solace to Robert Fawtier, who found himself writing *The Capetian Kings of France* under Nazi occupation:

This work . . . would probably never have been written had not the Second World War broken out . . . When the events of June 1940 overtook my country, . . . [i]n a time of national tragedy I found a source of strength, for myself and my audience, in the study of the beginnings of the French nation and of the actions of its first leaders. Such a study was, I found, not merely an escape from the horrors of contemporary reality. Like those monuments of antiquity which have provided the foundations for a whole series of later edifices, the original structure of France appeared to me so strongly built that it could not be completely destroyed. I hoped that something of this conviction might communicate

¹ Dana Carleton Munro, *The Middle Ages 395–1272* (New York, 1923), 158. Munro's preface carries a date two years earlier than the copyright, thereby explaining the usage of the text.

² William Stubbs, *The Constitutional History of England*, 3 vols. (Oxford, 1880), 1: v, 1.

itself to my readers, when my publisher was courageous enough to undertake publication. And even if his courage were to be ill-rewarded, the book itself would always keep for me the character it acquired during the two years in which it was my companion in adversity, the good Samaritan giving me succor in a parched land.³

Nevertheless, near-simultaneous textbook evidence demonstrates the extent to which the landscape of medieval political history was changing. In 1928, Raymond J. Sontag had written a new edition of Munro's *Middle Ages*, extending its coverage to 1500. By the end of the following decade, when further revisions were needed, Munro was dead and Sontag no longer interested. As a result, Joseph R. Strayer agreed to take over, and the third edition, now called Strayer and Munro, appeared in 1942, a year after Fawtier had completed *The Capetian Kings of France*.

While nineteenth-century historians had based their histories primarily on chronicles, Strayer (like Fawtier) turned to the close analysis of official records, documents that could be made to reveal the past with much more precision than mere chroniclers' tales. Unsurprisingly, then, his interpretation of the medieval political past turned out at times to differ substantially from Munro's, but nowhere more strikingly than in his expanded version of the passage in which Munro had lamented the consequences of the Italian dreams of Otto I and his successors:

Long after the failure of Otto's policy was apparent, German rulers stubbornly refused to admit that it was wrong . . . It is easy enough to show the disasters caused by these beliefs; neither Germany nor Italy could become a strong, united nation as long as they were linked together by the tenuous, yet unbreakable idea of the Empire. German emperors wasted their energy in expeditions to Italy, instead of building a strong kingdom in their own land. Italy could not develop a national life of its own because of constant intervention by the German ruler. Both countries fell behind the more nationalistic and better organized states of the West . . . But looking at the matter from the viewpoint of the tenth century, it is hard to see how Otto could have acted otherwise. No one then believed that the development of national states was the chief end of man.⁴

With that typically wry last statement, Strayer laid to rest the last vestiges of nationalistic Whiggery. For him, clearly, the present had an obligation to understand the past on its own terms,⁵ and if those terms remained largely political, few were about to challenge that judgment: for the medievalists who came to maturity after World War II, after all, Strayer's proved a dominant and inspiring voice. Thus, whether expressed in textbooks or sparkling scholarship, his opinions were not lightly to be disregarded.

AT THE SAME TIME, THE VERY NOTION that nation states are not the chief end of man is at least potentially subversive of any belief in the centrality of politics, and

³ Robert Fawtier, trans. Lionel Butler and Robin Adam, *The Capetian Kings of France* (London, 1960), vii.

⁴ Joseph R. Strayer and Dana C. Munro, *The Middle Ages 395–1500*, 4th edn. (New York, 1959), 153.

⁵ So good, in fact, did he become at this approach that the present author vividly recalls the testimony of a man who had first encountered Strayer discussing medieval peasant life at one of Princeton University's Alumni Colleges in the 1970s. "He almost made me believe in reincarnation," the alumnus reported. "He made it so real that he *must* have been there himself."

so it proved in the aftermath. By the end of the 1960s, a new generation of medievalists had come to the fore, and, without elaborating on all the other factors involved, it is perhaps enough to observe that they, like their contemporaries in other fields, tended to reject the study of political history as being overly narrow, not to say elitist. They, too, insisted that history should be written from the bottom up, and even though the often fragmentary nature of medieval evidence, not to mention the unique features of medieval society, ensured that their scholarship would differ substantially from that on more recent periods—time on the cross, for example, never became a quantitative issue, instead giving rise to streams of religious and spiritual studies—the fact remains that medievalists began to stress the lives of ordinary people and the fresh perspectives suggested by feminist inquiry. Political history appeared dead; the new social history had arrived; and by the 1970s its tenets could be found permeating the textbooks, in this case *The Western Experience* by Mortimer Chambers and others, first published in 1974:

For a long time historians emphasized politics as the activity that summarized a whole society and determined its course, a view reinforced by emphasis on the development of the national state; and that familiar perspective with its convenient chronological framework is generally maintained in this account.

But this book also reflects the influence of a distinctive kind of historical writing, commonly called “social history,” which has greatly affected historical understanding in the past twenty years or so . . . Modern social history . . . seeks to compensate for the fact that most historical writing has been about the tiny minority of the powerful, rich, and educated . . . The new social history aims to be as mindful of popular culture as it is of formal or official culture, as interested in the family as it is in the state, in living conditions as it is in political theory . . . Readers should find some of their preconceptions—about the past, about how societies are organized, and about how people behave—severely challenged.⁶

Be that as it may, the last ten years have seen yet another swing of the pendulum. The 1980s may have been the Reagan years, ones in which politicians rushed to join the new social historians in stressing the family and family values, but in medieval studies, at least, they have also seen a marked revival in the fortunes of political history. As the books under review make clear, however, it is not the political history of old. Rather, it is a history less interested in the origins of the national state than in the role of the family, more committed to economic realities than to purely political rhetoric, and fully aware of the extent to which, at times, the views of popular culture may be vastly more important than those of the intellectuals. In short, its approach to the past has been significantly enriched by the methodology and findings of the new social history, as well as by those of historical anthropology and other related disciplines.

This transformation cannot be fully grasped without a knowledge of political history as medievalists once wrote it. In France, because it was assumed that the only politics worth studying were those involving the state, scholarly publication favored regnal monographs and institutional studies. The assumption seemed to

⁶ Mortimer Chambers, *et al.*, *The Western Experience*, 4th edn., 2 vols. (New York, 1987), 1: viii–x.

be that, once the details of every reign had been pinned down and the specifics for crucial institutions established, the historian's task would be finished since, essentially, there would be nothing left to say.⁷ The chief purpose of these works was to show how, increasingly, the growth of depersonalized institutions had come to shape the state, a process quite independent of the quirks and foibles of those who happened to staff and to run them.

The history of England presented a similar picture, though with variant shadings. First, because most of its ruling houses—for instance, the Normans and Yorkists—had ruled much more briefly than had their Capetian and Valois rivals, monographic attention focused much more frequently on whole dynasties, not just on individual rulers. Second, although institutional studies enjoyed considerable popularity in England as well, again and again their chief purpose, inconceivable in France, was to trace the growth of constitutionalism, demonstrating how the frame of government itself had slowly come to channel and limit the arbitrary desires of willful kings. Lastly, because most scholars shared a firm belief that the rise of parliament had resulted from the king's need to gain consent for taxes, and because, moreover, they thought that the success of constitutionalism had depended on the full governmental participation of ever broader segments of the population, finances and parliament came to be seen as crucial elements in any political history of the later Middle Ages.⁸

SUCH CONCERNS, EITHER ENGLISH OR FRENCH, are surely not those of social history. On the other hand, in order to understand why historians of medieval politics should have begun to incorporate many of its new interests into their own work, one must first appreciate the extent to which both fields involve similar evidentiary challenges. Ordinary people leave few records on which to base their histories, but neither did the political elites until the very end of the Middle Ages. Because the story of the elites, too, can be established only after patient analysis of limited documentation, medievalists were blazing a methodological path for the social historians described in *The Western Experience* long before the social historians had learned how “to discover information previously thought unobtainable,” by showing how it was possible to glean useful data from such unpromising sources as the “minutes of meetings and petitions,” “coins; pots and lamps; fiscal records and price lists; [and] parish records of births, deaths, and marriages.” Lastly, insofar as medievalists, like social historians, have always shown a marked preference for writing that is “analytical other than

⁷ In the early 1950s, for example, both Charles H. Taylor and Édouard Perroy assured the author that the history of medieval France would be finished as soon as proper regnal monographs had been done on Philip Augustus, St. Louis, and Philip the Fair. One doubts whether Messrs. Baldwin, Jordan, and Strayer, the authors of those works, believed that when they wrote.

⁸ Here a quirk should undoubtedly be mentioned, the fact that medieval English financial records are much more plentiful than French ones, most of which were destroyed in the fire that burned down the Chamber of Accounts in 1737. One suspects that this difference helps to explain why English political history has always emphasized financial and economic factors much more often than does its French counterpart.

narrative in structure,"⁹ it could further be said that they were the ones who first solved the stylistic problem that use of such evidence presents.

That observation aside, the real point of the argument remains that, when evidence is scarce, its users come to know its content well. They may not use all of it, but even seemingly irrelevant details stick in their minds, a ready resource on which to draw whenever new purposes and questions arise. And that is precisely what has recently happened to the political histories of medieval England and France. When the revolution in social history suggested the importance of other questions and issues, medievalists found it relatively easy to review their data with a fresh eye, an activity that led to marked changes in their understanding of the whole political process.

These changes are best illustrated on the basis of personal experience. In 1966, when I wrote *The French Apanages and the Capetian Monarchy 1224–1328*, my avowed purpose was to study the causes and consequences of the royal decision to endow younger sons with large territorial fiefs, the so-called apanages. It might be thought, as a result, that its principal themes would have stressed the internal dynamics of the royal family itself, not those of government, but, in fact, its goal remained traditional: to gain insight into the monarchy by showing how apanages, one of its newly created institutions, had enhanced governmental effectiveness, thereby helping the king in his unremitting struggle to extend royal rule to all of France. If this was a family study, it was so only by accident.

Fifteen years later, when Andrew W. Lewis published his *Royal Succession in Capetian France: Studies on Familial Order and the State*, it proved to be a very different kind of work. In a sense, the differences resulted from Lewis's focus on earlier centuries, but they were much more profoundly a consequence of the context within which he viewed his sources—and of the uses to which he put them. In his frame of reference, the Capetians were first and foremost a family, and the evidence demonstrated the extent to which, even after gaining the throne, they had remained remarkably similar to other noble families in outlook, behavior, and goals. In particular, Lewis emphasized that all such families had faced difficulties in adapting themselves to the more hierarchical, nuclear, and patrilineal structure so newly characteristic of the era. He further argued that most of them had solved the greatest of those difficulties, that of preserving the integrity of family property, by insisting that the eldest son was to inherit all lands other than new acquisitions, the one kind of holding that cadets were permitted to enjoy.¹⁰

Given these findings, when Lewis came to consider the apanages, he saw little new about them. On the contrary, their creation reflected a practice that had long been followed by much of the nobility, and if there was anything at all that distinguished them, it was merely their size, the extent of which was less the

⁹ All quotations in this paragraph from Chambers, *Western Experience*, 1: ix, a discussion devoted to social history exclusively.

¹⁰ Andrew W. Lewis, *Royal Succession in Capetian France: Studies on Familial Order and the State* (Cambridge, Mass., 1981), 7–77.

result of new policy than of the enormous success of Capetian conquests during the reigns of Philip Augustus and Louis VIII. These were, in effect, the family's new acquisitions, and their transfer to younger sons simply treated them as such. Little wonder, then, that when Lewis turned his attention to some of the high claims so recently made about these grants, he could only remark: "The opinion which would make the thirteenth-century apanages a line of supports set up for the crown is . . . plausible, but the texts do not state such a thing and their ambiguity does not require this interpretation. Other explanations must be sought . . . in what is here conceived to be their context: the royal family."¹¹

In the Communications section of this journal, differences of this magnitude frequently lead to what diplomats call "a frank exchange of views" or, in less guarded language, to passionate letters of hurt, exception, and outrage. Yet nothing of the sort happened here, for Lewis was clearly right, and the very fact that the evidence was limited, and very familiar to all who had studied it, made it easy to see that his views flowed from a more accurate, because more comprehensive, analysis of the sources. And what made his views more comprehensive was his very ability to incorporate into his reading questions that derived, ultimately, from social history and its related disciplines. By bringing to bear studies on other families and then by testing their conclusions against the Capetian evidence, he was able persuasively to demonstrate that, whether in property transactions or even in family naming patterns, these kings of France had continued to behave much like their noble contemporaries. Because they had, Lewis found it possible further to argue that their primary political goal had been to maintain and enhance purely family resources, not to create the institutions of a lasting French state.

LEWIS'S CONCLUSIONS SUGGEST THAT, at the most general level, the central concerns of medieval politics differed markedly from modern ones, and his judgment seems confirmed, though in surprisingly varied ways, by other recent scholarship. To cite an obvious case in point, William C. Jordan has long displayed an interest in the impact of crusades on French royal administration, and in an early article he showed how practical crusading needs—for supply depots, for an adequate port of embarkation with proper roads leading to it—had virtually forced the government of Louis IX to reform itself in order to develop the resources and personnel needed to transform the king's dream into a reality. Jordan was certain that this kind of planning had had enduring consequences in terms of governmental effectiveness.¹²

Nevertheless, when Jordan came to write his *Louis IX and the Challenge of the Crusade* (1979), he, like Lewis, found that new questions led to fuller answers, ones that encompassed much more than the institutional interests of old. It

¹¹ Lewis, *Royal Succession*, 2.

¹² William C. Jordan, "Supplying Aigues-Mortes for the Crusade of 1248: The Problem of Restructuring Trade," *Order and Innovation in the Middle Ages*, William C. Jordan, *et al.*, eds. (Princeton, N.J., 1976), 165–85, 459–65.

remained true, of course, that crusading required detailed planning and the development of resources on a scale previously unknown, but it also became clear that the fundamental decision to go on a crusade at all was best understood within the context of Louis's need to gain autonomy from Blanche of Castile, his dominating mother, and of his fervent desire to emulate, insofar as possible, the religious example set by his saintly sister Isabella.¹³ In effect, Louis became a crusader not as a matter of state policy but because he found in crusading a way to express his own personal religiosity while at the same time freeing himself from maternal control.

While Aigues-Mortes still needed to be built as a port of embarkation, Louis's pre-departure plans had strikingly emphasized such unworldly concerns as the founding of religious houses, the purchase of the crown of thorns, and the construction of Sainte Chapelle to house it. Indeed, if, almost immediately after taking the crusader's vow in 1244, he had created commissioners (*enquêteurs*) and empowered them to give justice to all those wronged since Blanche had assumed the regency in 1226, his aims had scarcely been governmental or political in any modern sense. Rather, he hoped thereby to square things with God who, in return, would surely crown Louis's expedition to Egypt with success.¹⁴

When, on the contrary, this venture met with crushing defeat, and Louis himself was captured, the lesson learned had not been a practical one—that, for example, it was unwise to land troops in the Nile delta only weeks before the annual flood. Instead, it was wholly a religious one, that God had denied victory to this crusade because he remained dissatisfied with Louis's reforms, ones that had failed, as yet, to bring true justice to all of France. When Louis finally returned from Syria in 1254, he responded to this lesson by spending the rest of his reign reshaping his field administration, issuing ordinances designed to improve the quality of justice and the government of his towns, and in all ways trying to persuade his people to abandon sin. These are, of course, the actions that made Louis the founder of many of the Old Regime's crucial institutions, but in Jordan's account they take on a different character, one in which Louis's guiding intent was solely to please God so that his final crusade, against Carthage, would not fail. No politician would adopt such an approach today except, notably, Ruhollah Khomeini, a leader whose Islamic republic undergoes periodic purification to ensure that its strength will be that of Allah.¹⁵

To an English contemporary, Matthew Paris, Louis IX was "the king of mortal kings," high praise indeed from a chronicler not much given to the positive assessment of monarchs. Still, as different from other rulers as Louis may have been, the new political history underscores the extent to which even those others often found that the realities of medieval life encouraged patterns of political behavior far removed from those now thought of as typical. When the *AHR* reviewed Chris Given-Wilson's *Royal Household and the King's Affinity: Service, Politics, and Finance in England, 1360–1413*, Bryce Lyon first praised it as

¹³ William C. Jordan, *Louis IX and the Challenge of the Crusade* (Princeton, N.J., 1979), 3–13.

¹⁴ Jordan, *Louis IX and the Challenge of the Crusade*, 35–97.

¹⁵ Jordan, *Louis IX and the Challenge of the Crusade*, 135–81.

“financial and administrative history in the tradition of Tout,” the kind of “old-fashioned history [that] is virtually sneered at by the new breeds of historians who preach that the only relevant and redeeming history is that orientated to social problems.” Understandably, then, when he discovered that Given-Wilson saw some importance in the fact that one of the duties of the chief marshal was “to employ twelve *damoiselles* who were prostitutes,” he went on to add: “It can be concluded, therefore, that something like an organized brothel was associated with the royal household. Other details, mostly gleaned from the wardrobe books, inform us about minstrels, musicians, gambling, chess, banquets, archery, tournaments, hunting parties, and purchases of luxury items such as jewels and garments. And we learn down to the lowest fraction of a penny how much it cost the king to keep his dogs, falcons, and hawks . . . Yes, even this monograph cannot escape a little social history.”¹⁶

For Lyon, indisputably a scholar of the old school and proud of it, such information, while colorful, is basically unimportant. It has little to do with political and institutional history as he understands it. Given-Wilson has a different view. He finds it important to discuss the evolving spatial arrangements of royal residences but not because he is interested in architecture as a social phenomenon; rather, he knows (as do students of the modern White House) that the decision of who goes where has a lot to do with who speaks to whom, above all to the king. Similarly, if Given-Wilson worries about the extent of royal alms-giving and the cost of ostentatious display, it is because he recognizes that both helped to elevate the royal majesty even as they provided forms of patronage that encouraged loyalty among those whose duty it was to keep the wheels of government turning. Lastly, insofar as one of the measures of governmental effectiveness is the extent to which basic infrastructure is maintained, even the royal falcons and dogs take on relevance when one finds that “in October 1373 the sheriff of Oxfordshire was ordered to repair all the bridges in his county at once ‘for the king’s sport with his hawks in the approaching winter season.’”¹⁷ As Given-Wilson himself sums up the most important of his conclusions:

To provide a suitable milieu for the ruler was . . . one of the primary functions of the royal household and beyond this, the importance of the household lay really in its flexibility rather than its institutionalisation. The idea of “household government,” that is, of the household as an administrative haven wherein the monarch might escape the vigilance of barons or others who wished to reform his government, has little relevance . . . The household was a centre of social life for the nobility of the kingdom; it was a point of contact, between central and local government, and for those who wished to lobby the king or his courtiers; it was the nucleus of the king’s army; and it maintained at all times a substantial reserve of both cash and weaponry, which might prove extremely useful in times of crisis. In this way, it was an essential adjunct to kingship.¹⁸

¹⁶ Review by Bryce Lyon of Chris Given-Wilson, *The Royal Household and the King’s Affinity: Service, Politics and Finance in England 1360–1413* (New Haven, Conn., 1986), *AHR*, 92 (December 1987): 1191–92.

¹⁷ Given-Wilson, *Royal Household*, 62.

¹⁸ Given-Wilson, *Royal Household*, 260.

THE MOST STRIKING ASPECT OF THESE FINDINGS is Given-Wilson's insistence that, as late as the fourteenth and early fifteenth centuries, the English royal household remained fluid in form, adaptable in function, and much less institutionalized than Tout once led us to believe, notably in *The Place of the Reign of Edward II in English History* (1914). Even the supposed tyranny of Richard II takes on new form, for no longer is it the result of an over-reliance on household government and the oppression of his Cheshire archers, also a part of that household. Richard's failures, like those of other rulers, can be interpreted as failures in policy, and his inability to lead men as much more important than what had hitherto been viewed as their cause, his unwise use of patronage to reward favorites at court while at the same time denying favors to resentful power brokers at the local level whose resultant hostility was ultimately to do him in. As Given-Wilson shows, however, Richard II, like Henry IV after him, was an attentive patron of men whose standing reflected the realities of local politics, realities that help to explain just why it was that so many of those who had received the deposed king's bounty continued to enjoy the gifts of his usurping successor.¹⁹

These views are echoed and confirmed by findings across the Channel. To the old school of politics, institutionalization was important because institutions tend to take on a life of their own, and because they do, a government that is fully institutionalized is less subject to the whims of capricious human beings. Such a government follows proper procedure—the rule of law—rather than the willful fancies of those who, for the moment, control it. Still, for all the attractiveness that this vision of depersonalization may have, it suffers from the obvious difficulty that it leaves no one in a position to be responsible for the successes and failures of government.

In French history, nowhere was this difficulty more apparent than in the reign of King Philip the Fair, 1285–1314. In the traditional accounts of those years, there were momentous developments—outward expansion toward France's "natural" frontiers, the creation of the Estates General as an expression of the will of "the nation," the intensification of taxes, and the resolute denial of all potentially supranational authority such as that of the pope. Yet the extent to which Philip deserved personal credit or blame for these developments remained obscure. Almost without exception, chroniclers insisted that he had been a shadowy enigma, uninvolved in the normal duties of royalty, and these claims seemed not implausible to many historians. After all, institutionalists had long assumed that by 1300 governments had become highly bureaucratized, so it was easy to believe that this king had wholly removed himself from all policy decisions, not to mention the daily administrative cares of those who ruled in his name.

Just three years before his first revision of Munro's textbook appeared, Strayer showed himself to be sympathetic to such an interpretation. Because the nature of his work in 1939 did not require him to decide, in his words, whether "Philip

¹⁹ Given-Wilson, *Royal Household*, 200–01, 233, 254–60.

the Fair personally directed the policy of his ministers," he tried to avoid the question by ascribing all actions to a cautiously evasive "government," but he then added: "In spite of this attempt to be neutral my own beliefs may have crept into the text and I should warn the reader that I feel, at present, that Philip had little control over his ministers and that many important acts represent the desires of the bureaucracy rather than those of the king."²⁰

When Strayer later returned to the problem, he found that prolonged immersion in the records of the reign (as well as, one suspects, his own governmental experience) had led him to quite the opposite conclusion. In a justly famous *AHR* article, one that formed the basis for a crucial chapter in *The Reign of Philip the Fair*,²¹ he argued that Philip had been the man responsible after all. In proof of the point, he cited not just the frequency with which documents show the king to have been personally involved even with trivial matters but also—and even more fundamentally—the continuity of basic policy that had persisted as one minister succeeded another, often with bewildering rapidity. Given the range of personalities involved, and further given the demonstrable differences of every variety that distinguished the outlook and goals of those whom Philip selected to serve, it was not to be believed that these men could have worked together so harmoniously, could have succeeded each other so smoothly, or could have maintained the same policy objectives so consistently unless the king himself had been very much in charge.

As analyzed by Strayer, the political realities of Philip the Fair's government seem remarkably similar to those of Given-Wilson's English household. In neither instance is there much sign of significant institutionalization. Rather, in both countries, governmental effectiveness depended on the personal abilities of the king, on the interest he took in the practical administration of his realm, and on the extent to which he was able to translate his own desires into politically useful action, something that required constant interchange between the king, his officials, and the notables of his realm. Direct human contact was the key, but that, in turn, demanded a royal personality, a royal presence, impressive enough to persuade others that it was undoubtedly in their own best interest to follow his lead.

There is, however, another side to the story. In relaxed moments, though never in print, Strayer used to regale his friends with an expansive disquisition about the one book he remained eager to write, on the king as real-estate agent. With the considerable help of Strayer's impish imagination, medieval rulers would find themselves transformed into little more than the mergers-and-acquisitions specialists of their day, masters of the Monopoly board of life whose political successes could be measured precisely by deducting the square mileage of the estates they had been forced to cede from that of the ones they had been able to acquire. Only those kings who achieved a positive balance were to be

²⁰ Joseph R. Strayer and Charles H. Taylor, *Studies in Early French Taxation* (Cambridge, Mass., 1939), 4 n. 2.

²¹ Joseph R. Strayer, "Philip the Fair—A 'Constitutional' King," *AHR*, 62 (October 1956): 18–32. In revised form, this article became the first chapter of *The Reign of Philip the Fair* (Princeton, N.J., 1980).

deemed successful, whereas those who did not could not. Like John Lackland (a favored example in the wee morning hours), these men were failures whose governmental difficulties had arisen from their realty losses. And, if auditors expressed doubt on the point, an amused Strayer would lightly assure them that they had yet to grasp the extent to which the harsh nature of economic reality had dictated the one set of rules by which the medieval political game could be played and won.

Although Strayer's death means that this book will remain unwritten, recent research has sustained his judgment. In effect, medieval kings constantly confronted the politics of scarcity, and their actions are largely to be explained by that context. Limited resources—and hence the competition for them—always intensify the normal conflicts of political life, and in the Middle Ages those conflicts were unusually severe because resources were themselves so scarce, consisted largely of land, and could be lawfully enjoyed only with the consent of a king whose position at the apex of the so-called feudal pyramid allowed him to decide who, as of right, was to have the fruitful possession of landed property.

STILL, IF TO EVERY KING THERE WAS A SEISIN, others were anxious to prove that there could indeed be something new under the sun, a world in which vassals would enjoy more of that seisin, and enjoy it unencumbered by any responsibilities to the man at the top, a person whose sacring had transformed him, in theory at least, into a vicar of God who sometimes claimed that his word had the force of law. It was undoubtedly more blessed for him to give than to receive, but the scarcity of resources encouraged kings, in response, to use their position to gain every possible advantage out of the gifts they made. As W. L. Warren puts the case on the opening page of *The Governance of Norman and Angevin England 1086–1272*:

Nowadays we tend to think that the kind of society any state has is determined by the type of government it has. In the Middle Ages the type of government it was possible to have was largely determined by the kind of society over which it was trying to rule. Government was more about controlling and managing society than about changing it. It is true that government developed at the will of enterprising rulers; but a successful ruler was like a gardener training an apple tree: he might prune and cut back, select and encourage, but he could work only with what was already there. It is proper to begin not with the structure of government but with the structure of society.²²

In the rest of the book, Warren then follows his own advice, though nowhere more notably than in his discussion of the place of shires in Anglo-Norman government. The purpose of organized shires was not to govern the realm but to have an efficient means for collecting whatever was owed to the king. Moreover, because there was an amazing diversity in the organization of shires, royal government found itself more shaped by the varied structure of shires

²² W. L. Warren, *The Governance of Norman and Angevin England 1086–1272* (Stanford, Calif., 1987), 1.

than vice-versa. And, although it is true that under the Normans the shire began to change from an autonomous entity “into an administrative subdivision of the realm,” Warren maintains that this gradual transformation, based largely on an ignorance of how Anglo-Saxon shires had actually worked, provoked a crisis in local government under Henry I, a crisis that Stephen made considerably more dangerous by turning shires into effectively independent earldoms. This, then, was the basic challenge to which Henry II and the Angevins were responding, and although John’s reign ended badly, Warren sees Magna Carta less as a charter of liberties than as an extended critique of all of Angevin government.²³

Given the scope of Warren’s concerns, his study is to some extent a synthetic overview, but his conclusions about the essential dynamics of medieval government in the making are fully confirmed by the detailed work of Judith A. Green and, in France, John W. Baldwin. In a sense, Warren’s argument is like Strayer’s: that the crucial issue for any medieval government was resources, their proper management and allocation. As a result, the growth of what we think of as government, particularly in the twelfth century, might better be termed the creation of a whole series of administrative procedures, the chief purpose of which was not to govern but simply to maximize the rate of return on available resources.

IN *The Government of England under Henry I*, GREEN devotes the bulk of her space to precisely this issue. Through a minute analysis of royal finances based largely on the Pipe Roll of 1130, and through an equally detailed study of the careers of 104 men then in governmental service, she attempts to illuminate the nature of the central government that began to take shape independent of Henry’s household. Her findings are instructive. Gone are Oderic Vitalis’s familiar “men raised from the dust,” their place taken by others who were equally new but of a quite different variety. Far from impoverished, they were typically drawn from the lesser landholding families. They often had experience, a trait that Henry appeared to favor, and were also remarkably professional in outlook and shared a capacity to employ all of “the more sophisticated techniques” of an increasingly literate age in the service of their king. And, although that service was to make Henry “the Lion of Justice,” a quality not without its importance for later ages, at the time its principal aim was to enhance revenues by rationalizing and strengthening the means by which they were gained.²⁴

A similar picture emerges in John Baldwin’s *Government of Philip Augustus*. In the traditional view, Philip’s claim to have been the true founder of the French monarchy rested on two premises, both of them related to the success of his struggles with John Lackland. Because he had conquered Normandy—indeed, all Angevin lands north of the Loire—he had thereby become overwhelmingly the largest landholder in France, no longer merely the first among equals. But

²³ Warren, *Governance of Norman and Angevin England*, 25, 30–31, 63, 72–73, 164–69.

²⁴ Judith A. Green, *The Government of England under Henry I* (Cambridge, 1986), chaps. 4–7 and pp. 216–18.

of even greater importance was his encounter in Normandy with a whole set of governmental institutions, the range and sophistication of which had no counterpart in royal France. As duke of Normandy, the king of England enjoyed, and had the bureaucratic personnel actually to realize, powers that far exceeded what any Capetian had ever achieved. With these Norman institutions as a model, Philip soon so transformed the nature of his own government that by the end of the reign he could truly be termed the king of France.

Like Green, Baldwin has immersed himself in the documents, frequently resorting to the kind of computerized analysis that is the mainstay of social history. The result is a very different picture of Philip's accomplishment, one in which the key to success lay not in the imitation of Norman models or the simple acquisition of new lands. On the contrary, its origins lay in the early years of the reign and in the patient ways in which Philip sought to improve the administrative efficiency (and hence the revenues) of what he already had. If, by 1204, he had the resources to overmatch John, it was because he had managed by 1190 to squeeze 72 percent more income out of what he had inherited than his father Louis VII had ever enjoyed—and with the reorganization mandated in Philip's will of 1190, an event that Baldwin quite rightly sees as a turning point, he had then gone on to add another 50 percent by the time of his budget of 1202–1203 or, in other words, the time of his condemnation of John.²⁵ This is government, surely, and political manipulation was never absent from it, but insofar as it involved a remorseless totting up of accounts (a chore for which, Green implies, most medieval kings had little time or inclination),²⁶ it is government and politics far removed from the assumptions of Dana Carleton Munro and his generation.

A decade ago, Warren Hollister and John Baldwin used these pages to argue that the proper term for this form of rule was “administrative kingship,” a type of government in which feudalism—that tyrannous construct against which Elizabeth A. R. Brown has quite properly inveighed²⁷—finds itself replaced by a much more dynamic process, one in which the constant interaction between kings and their vassals encouraged rulers to create new forms of administration that could wrest more income out of all available assets. The result was not government in any modern sense, surely, but, like the return of Martin Guerre, this refashioning of medieval politics is likely to have consequences for others, notably the textbook writers of the next generation.

²⁵ John W. Baldwin, *The Government of Philip Augustus: Foundations of French Royal Power in the Middle Ages* (Berkeley, Calif., 1986), 44–58, 144–64, 174–75.

²⁶ Green, *Government of England under Henry I*, 1.

²⁷ C. Warren Hollister and John W. Baldwin, “The Rise of Administrative Kingship: Henry I and Philip Augustus,” *AHR*, 83 (October 1978): 867–905; Elizabeth A. R. Brown, “The Tyranny of a Construct: Feudalism and Historians of Medieval Europe,” *AHR*, 79 (October 1974): 1063–88. See also Warren, *Governance of Norman and Angevin England*, 24, where he explains why he, too, avoids the term.

Reviews of Books

GENERAL

THEODORE S. HAMEROW. *Reflections on History and Historians*. Madison: University of Wisconsin Press. 1987. Pp. xiii, 267.

The role of history in our culture and general outlook, not to say our curriculum, has recently received a considerable amount of anxious attention. Joining reports on the sorry state of the humanities and "cultural literacy" have come more specialized assessments of history by Gertrude Himmelfarb, Michael Kammen, and others. Now Theodore S. Hamerow joins this company with a sobering examination of academic history, including its certification and support systems.

His is a thought-provoking and poignant book, written in a reflective and even elegaic vein. It will send shudders—but some laughter too—through the most hardened veteran, inured to periodic bouts of collegial soul-searching. The author contends that academic history is in the most serious crisis of its relatively young life and that its chances of survival are poor. Major changes are necessary but unlikely, so we must face the inevitable question: what if history as an academic discipline should disappear? What does historical awareness mean to the social (and personal) order? Hamerow addresses these fundamental questions with such sustained insight and commitment that anyone even remotely concerned with history will be moved by his discussion. Some practitioners will be either engrossed or enraged by his judgments regarding the vanity of much academic history.

Despite the somber nature of his task, the author is neither dry nor lachrymose. In conveying the "feel" of the discipline, the experience of scholarship, and the "way of life" of higher education, he combines vividness with candor. If style alone could salvage history, Hamerow would have won the major battles: he adeptly integrates revealing personal observations with broader analyses, including statistical data. The work proceeds topically and historically in a way that recapitulates both the collective development of the profession

as a whole and that of an individual historian in apprenticeship. After a ringing statement of the "crisis," there follow chapters on professionalization, graduate training, "history as a way of life," the "new and old histories," and a concluding chapter on the uses of history in its current time of troubles. There are two appendixes containing synopses of the views of prominent "popular" historians and of representatives of major foundations toward academic history.

The author finds that academic history in its current form is headed toward a kind of extravagant extinction. Despite spasms of flashy and telegenic docudrama, our culture is becoming ahistorical in its basic outlook. Under the spell of the social sciences, which reflect and flatter its current preoccupations, society has come to value instant predictive knowledge as what counts and what pays off. Students are increasingly preprofessional and interested only in the up to date, not to say the market in futures. Likewise, the "star" professional historians have tended to become empire builders, fad chasers, and grant-grubbing team members. Almost everywhere quality has given way to quantity: in class size, number of articles, fellowship awards, and, most important, in methodology and in the notion of historical evidence itself. On many fronts academic history is disqualifying itself from what Hamerow sees as its "indigenous" human interest. Its methods are running contrary to its claims and purposes.

Hamerow pursues his case by adopting both a long view of the profession and an intimate view of daily life in academe—not exactly the historiographical *durée* and everybody's *vie quotidienne*, perhaps, but essentially covering the last century and the life cycle of the professor. These spans provide perspective but also raise problems of emphasis in his argument. Hamerow is more pessimistic than he needs to be in the light of some recent academic trends that he neglects. The present is neither as obliviously nor studiously "presentistic" as he makes it out. What about the revival of narrative in academic history, the intel-

lectual respectability of biography, and the "new historicism" in literary and humanistic study? And what about the call for more rigorous, undiluted history in the core curriculum? Hamerow's critique of the new social history—as well as his portrayals of the "typical" professional meeting and departmental party—are lopsided, if colorfully caustic.

Hamerow's message is meant to be unsettling but, unintentionally, so are some of his procedures for getting it across. There are ten tables of data and sections of sociodisciplinary analysis (bolstered with AHA inquiries and presidential addresses over the years), but some of the data seem so old or ill-suited to the basic question of contemporary "crisis" as to be marginal—as if the author were arming himself with the archaic cast-off instruments of the enemy ("hard" social scientists) and the quaint slogans of venerable allies ("soft" genteel humanists). The reader may at times feel caught in an infinite regression; the author seems to take special delight in pressing theoretical debates back to their earliest origins in order to deflate claims to innovation on the part of "new" historians, whom he regards as more impressive in theory than in practice. But this tactic can backfire by also being directed to the very notion of an unparalleled crisis in history; like "new" histories, "crises" in the discipline have been around for a long time. Indeed, some of Hamerow's arguments about current conditions and attitudes seem already "historical," that is, dated. By the same token his hints about the "history of the future" need further elaboration if they are to offset his critical account of recent practice.

Talk of the decline and even demise of history has been rife for as long as its professionalization; Hamerow could be read to suggest that these may even be concomitant developments. In any case there is no way to freeze history cryogenically in some mold until better times and then bring it out for action. As long as there are professionals getting a hearing who can theorize and practice as acutely as Hamerow, his eulogy seems premature.

MICHAEL ERMARTH
Dartmouth College

BRUCE A. KIMBALL. *Orators and Philosophers: A History of the Idea of Liberal Education*. Foreword by JOSEPH L. FEATHERSTONE. New York: Columbia University Teachers College Press. 1986. Pp. xix, 293. \$19.95.

Although the unexamined liberal education is not worth pursuing, examinations of liberal education are often less than illuminating because they appeal to an illusory consensus as to the meaning of

the term. Bruce A. Kimball has traced the historical development of the idea of liberal education to elaborate a typology useful in clarifying contemporary discussion of the subject.

This typology hangs on the identification and description of two major and antithetical traditions regarding the nature and goals of liberal education: the oratorical and the philosophical. The oratorical tradition, traceable to Isocrates, flowering among the Romans, and informing the vision of the Renaissance humanists, Matthew Arnold, and many present-day teachers of humanities, Kimball refers to as the "artes liberales ideal," identifiable by seven characteristics: "(1) Training citizen-orators to lead society (2) requires identifying true virtues, (3) the commitment to which (4) will elevate the student and (5) the source for which is great texts, whose authority lies in (6) the dogmatic premise that they relate the virtues, (7) which are embraced for their own sake" (p. 228). The philosophical tradition, the "liberal-free ideal," in Kimball's terminology, is ultimately traceable to Socrates and Plato but emerged in strength in the thought of Enlightenment philosophers and finds expression in the great research universities. It also is identifiable by seven characteristics: "(1) Epistemological skepticism underlies (2) the free and (3) intellectual search for truth, which is forever elusive, and so all possible views must be (4) tolerated and given (5) equal hearing (6) with the final decision left to each individual, (7) who pursues truth for its own sake" (p. 228). In response to the liberal-free ideal there soon arose what Kimball terms the "artes liberales accommodation" (neohumanism), emphasizing the primacy in education of intellectual training rather than the cultivation of noble virtues, defending classical letters on the ground that they enhance freedom and equality, and endorsing canons of specialized research. Later there emerged a "liberal-free accommodation" (meritocratic research specialization) placing some limitations on freedom in response to pressures for conceptual uniformity, institutional standardization, and curricular utility.

Intellectual historians will find much in the book that is admirable and useful. Kimball severely limits the claims to be made on ancient Athens as the generator of the theory and practice of liberal arts education and does a workmanlike job of tracing and untangling the roots of his typology in the Middle Ages and the period of the Renaissance and Reformation. By far his most useful chapters are those that trace the elaboration of educational ideas and practices in the United States. The book's greatest drawback is the lack of reference to any social or economic causes or

conditions of the educational ideas and practices discussed.

We may be justly suspicious of the notion that complex ideas with a history measured in millennia may so satisfactorily yield a typology clearly expressed in two major emphases, each reducible to seven hard and fast characteristics and each generating a single accommodated form. Fortunately, Kimball is sensible of the limitations of models and is not intent on making his a Procrustean bed to which every educational theorist and theory must be fitted. In fact his typology does yield a good measure of the clarifying influence he claims, and it will be a shame if his book does not gain the attention it deserves during the current and continuing debates on educational reform.

JOSEPH M. MCCARTHY
Suffolk University

RUTH W. GRANT. *John Locke's Liberalism*. Chicago: University of Chicago Press. 1987. Pp. viii, 220. \$24.95.

The most important recent Locke scholarship locates the philosopher's achievement within the local contexts of his era. It connects Locke's arguments to his circumscribed intentions and clarifies these intentions by placing them within a contemporary setting of religious controversy and political revolution. Ruth W. Grant's graceful and measured study profits from this work, particularly that of John Dunn, James Tully, and Richard Ashcraft, but it has a different purpose. Returning to a prior analytical tradition, Grant argues for the coherence of Locke's political philosophy, seen as a classic defense of liberal government, and seeks to ground his politics in *The Essay Concerning Human Understanding*.

Locke's main purpose in the *Essay* was to guide rational beings in the domains of knowledge that concerned conduct and belief. A satisfying feature of Grant's work is her attempt to relate this exoteric and practical enterprise to *The Second Treatise of Government*. She argues that Locke sought to give a reasoned proof of the extent of rights and duties and, in addition, to demonstrate the obligation of disputing parties to obey a common impartial judge. Grant focuses on Locke's discussion of mixed modes and relations in the *Essay* and on the important connection in Locke's thought between styles of moral reasoning and forms of political understanding. She makes an interesting case for the view that the *Second Treatise* is a work of political ethics, rather than a policy tract, and that Locke's strategy of avoiding historical argument in that work can be seen to follow from his epistemological claims about the possibility of normative

theory. Grant's deft handling of the texts, including unpublished manuscripts, makes this the most rewarding section of her book. As she concedes, however, Locke's discussion of ethics is incomplete, and Grant's account might have been stronger had she attended directly to his demolition of innate cognitive dispositions ("innate ideas"). This feature of Locke's argument addressed the most pressing religious and moral questions of his day. Moreover, it was found by his critics, Henry Lee, Edward Stillingfleet, and William Sherlock, as well as by his Enlightenment followers, to be the *Essay's* radically anti-authoritarian element.

Historians may find the remainder of this book less engaging because it is, in effect, a rational reconstruction of the normative components of Locke's political thought rather than a historically embedded account of its development. Grant employs considerable interpretive tact when evaluating Locke's conception of legitimacy, obligation, consent, and revolution, seeking in each case to resolve what she claims, perhaps too strongly, are only apparent or insignificant tensions in his argument, which she then judiciously criticizes. The concluding account of what she views as Locke's anxious political realism is, moreover, subtle and generous, and its focus on the *Essay* enriches the analytical tradition of understanding Locke's politics. Yet Grant presumes throughout her study that Locke sought first to defend and then enlarge "liberal political theory," a conceptual entity that neither he nor his contemporaries would have recognized as animating their concerns.

E. J. HUNDERT
University of British Columbia

J. B. HARLEY and DAVID WOODWARD, editors. *The History of Cartography*. Volume 1, *Cartography in Prehistoric, Ancient, and Medieval Europe and the Mediterranean*. Chicago: University of Chicago Press. 1987. Pp. xxi, 599.

Planning for this work stretches back to 1975, and the appearance of the first volume has been eagerly awaited. Expectations are not disappointed. Following J. B. Harley's historiographical introduction, nineteen chapters by nine contributors range from the origins of cartography through the prehistoric period, the ancient Near East, Egypt, Greece and Rome, to medieval Europe (ending ca. 1500). The writing is lucid and splendidly illustrated, even if the block of forty color plates could be better integrated with the text. Since bibliographies are offered at the close of each chapter as well as at the end of the volume, attention might also have been given to adopting briefer forms of citation in the copious footnotes.

Appropriate stress is laid on the importance both of the fresh attempt "to understand the developments in various periods on their own terms" (p. 342) and of the need to grasp the purpose of any map in order to evaluate it (p. 284). C. Delano Smith's sensitive appraisal of maps in the prehistoric period is outstanding in these respects (chap. 4), as are the chapters (18–20) on the three types of medieval map (all essentially unrelated), in particular T. Campbell's reexamination of portolan charts—the first in English for almost a century. More questionable are the reconstructions of certain ancient cartographers' maps (for example, figures 8.5, 10.8, 9), about which editors Harley and David Woodward justifiably express general reservations (pp. 286, 503). Consequently, it is valuable to have O. A. W. Dilke's explanation of why Agrippa's map cannot be reconstructed in detail (but note now the cautious sketch by R. Moynihan, *Archaeologia Transatlantica* 5 [1985]: 162).

Equally encouraging is the recognition that "for long periods—and in probably the greater part of the Mediterranean and European regions—very few maps were being made or used" (p. 503). In this connection the level of map consciousness in the Greek and Roman worlds seems overestimated by both the editors (p. 508) and the relevant contributors, G. Aujac and O. Dilke (see especially pp. 157–60 and chap. 16). Dilke's substantial and very learned contributions to the book overlap with his *Greek and Roman Maps* (1985). As I indicate in a review of the latter (*Journal of Roman Studies* 77 [1987]: 210–12), Ionian coins are wrongly interpreted as portraying maps (pp. 80, 158), and there never was a "civil service maps and plans department" (pp. 244, 252) in the Roman Empire. In addition Dilke seems too ready to credit the tale of late, romanticizing sources (add the Ebtorf map, p. 309, n. 111) that a "survey of the known world" was completed at Julius Caesar's instigation (pp. 205–06). Admittedly, he is more cautious—but still not skeptical enough—about the new stone "map of Gaul" (figure 12.5). As he demonstrates, Romans did survey extensive areas most professionally, but there is no sign that the huge collection of plans deposited in Rome was ever much exploited by the state.

Thus, the "paradoxical" contrast that Dilke finds between Roman map consciousness and Byzantine lack of it seems largely illusory. Instead, what emerges from the sweep of this survey is a striking uniformity over the whole span up to 1500. All the literate peoples of Europe and the Near East preferred to set out and record topographical relationships by written descriptions, rather than graphically: that applies as much in ancient Babylon and Egypt as it does in the Middle

Ages. Even Ptolemy, whose contribution to the development of map projections was to be so formative, may not have drawn maps at all, and *mappaemundi* were "as much written as drawn" (p. 325). The absence of "general maps designed to be put to a wide variety of uses" (uses considered obvious today) is similarly as blatant in prehistoric times as it is in the Middle Ages. Dilke especially appears too quick to attribute modern attitudes to Romans and modern functions to their maps. The main purpose of Gracchus's map of Sardinia, for instance, was surely religious, not political or practical (pp. 205, 254). The senate's lack of interest in the location of campaigns, as now argued by J. S. Richardson, *Hispaniae: Spain and the Development of Roman Imperialism, 218–82 B.C.* (1986) seems characteristic.

Such uniform absence of map consciousness need hardly surprise us. In all these societies there were no means to measure time or distance with unfailing accuracy whenever required, let alone standard units in which to record them. Nor was there writing material with qualities to match those of, say, Chinese silk. Identical copies could not be produced either. Of course many advances were made by ancient scientists, and their work continued to be read, but there was only limited interest in applying it, and among the Church Fathers a positive aversion (p. 326). Thus, difficulties of the kind faced by Spanish armies on the march even in late sixteenth-century Europe (as set out by G. Parker, *The Army of Flanders and the Spanish Road, 1567–1659* [1972]) are precisely those of ancient and medieval commanders. Likewise, the rudimentary state of mapping immediately prior to the development of modern cartography in Europe is now well outlined by J. W. Konvitz, *Cartography in France, 1660–1848* (1987).

Among the major issues thrown into sharper relief are how portolan charts originated (as is acknowledged, the "traverse-trilateration" theory on pages 387–90 is not a full explanation) and why the idea of mapping grew so dramatically after 1500 (p. 465). But the greatest challenge stems from the demonstration that before this date almost everyone in the regions covered was not map conscious: what, then, was the nature of their "mental map"? Although P. Janni has started to probe this question, his book can only be a beginning. The light shed on other cultures and periods by the five further volumes of this work is certain to help. The completed set will establish cartography as an indisputably important discipline in its own right, as well as forging for it stronger links with the wide range of related disciplines.

RICHARD J. A. TALBERT
McMaster University

WILLIAM D. PHILLIPS, JR. *Slavery from Roman Times to the Early Transatlantic Trade*. Minneapolis: University of Minnesota Press. 1985. Pp. xi. 286. \$25.00.

William D. Phillips, Jr.'s, work has two major objectives: to correct the mistaken view that European slavery died with the ancient Roman empire and to show how the main features of New World slavery were, in all critical respects, continuities of patterns that can be traced back to Roman times. Drawing mainly on the specialist literature, Phillips supplements his statements with illustrations from the more accessible primary sources.

Part 1 examines the nature of slavery and slave societies and slavery's rise and decline in ancient Rome. This part, relying too heavily on Moses Finley, overstates the role of labor demand in explaining large-scale slavery. A more careful attention to works emphasizing warfare and labor supply would have added balance to the synthesis.

Part 2, on medieval Europe, the Islamic world, and Africa up to 1650, shows how a diminished form of slavery persisted throughout medieval Europe. It reflects the current bias of the specialist literature in concentrating on Central and Southern Europe, unwisely neglecting Scandinavia and England, for which there is growing evidence that slavery was highly advanced. The discussion of Islamic slavery focuses on three aspects important for the later development of Western slavery: the diffusion of sugar cultivation; the relationship between Christians and Muslims in the Iberian peninsula; and the trans-Saharan slave trade. This is the least satisfactory section of the work, containing statements that are flatly incorrect, for example, the assertion that the Islamic world was "a vast economic unit" (p. 67). It is followed, however, by an excellent review and discussion of the development of sugar production in the Mediterranean islands showing the critical role played by Italian merchants in its production as well as the European slave trade.

Part 3 succinctly explores the spread of slave-grown sugar from the Mediterranean to the Atlantic islands and its diffusion from there to the New World by the Portuguese and Spanish, the early transatlantic slave trade that provided the needed labor, and the early pattern of urban and rural slavery in Latin America.

Phillips too often attempts to reconcile antithetical positions or to gloss over serious differences. Thus, he writes approvingly of modern definitions of slavery that emphasize power and sociocultural alienation while maintaining the traditional legalistic conception of the institution as property in persons. Phillips seems unaware of the serious difficulties created by the Western legalistic conception of slavery when considering non-Western

cases such as sub-Saharan Africa. Again, toward the end of the work he quotes at some length Philip D. Curtin's severe criticism of the continuity thesis—the fact that the new capitalist context made of the institution something "vastly different" from its Old World variant—but in the next, and final, chapter proceeds, without comment, to restate his view that the New World institution "was a logical outcome of patterns well established in Europe." Phillips is also not sufficiently alert to the dangers of defining slave society in the materialist terms of Finley and Pierre Dockès. If, as was true of many Islamic states, slaves largely led, administered, and defended a society and were critical in preventing it from falling into political chaos, the result was a genuine slave society even though few slaves were involved in production.

In spite of these shortcomings, this is an informative and lucidly written survey of an extremely important aspect of European history willfully neglected by British and American historians and teachers.

ORLANDO PATTERSON
Harvard University

JOHN KEEGAN. *The Mask of Command*. New York: Viking. 1987. Pp. 368. \$18.95.

This book is about the nature of generalship, the command and direction of armies in war. John Keegan is only marginally concerned with the mechanisms of operations and staff structures; his focus is the personal qualities of individual commanders. Although he concedes that successful generals share certain characteristics—energy, drive, and the will for victory—he rejects social science theories that exercising the same basic functions will always produce the same type of leader. He argues that military command always had to reflect the distinct ethos, traditions, and culture of the society that created a specific army. Therefore, quite apart from individual traits, successful military leaders displayed different persona, the outward manifestation of their personalities, a specific "mask of command" to which their particular armies responded. Keegan illustrates his ideas on the changing but interrelated nature of command, society, and war by examining the career and style of Alexander the Great, the duke of Wellington, Ulysses S. Grant, and Adolf Hitler.

Keegan classifies generalship according to two basic models: the heroic and the directorial. He notes, however, that the break between the two styles never became absolute, and, when circumstances demanded, even directorial generals had

to expose themselves to danger in order to manage their battles. These ideas, of course, are not wholly original, but, writing with his accustomed elegance and his usual flair for making resounding historical and cultural connections, Keegan has written a thought-provoking book on the exercise of command at the highest levels.

For Keegan, Alexander personifies the archetypical heroic leader. King, high priest, and general, Alexander shared the dangers of the common soldier and, dressed in shining armor, a brilliant cloak, and a plumed helmet, always fought in the forefront of battle. Although he forfeited a certain amount of tactical control, he played the role expected by his army and his culture. By contrast Wellington, Keegan's "anti-hero," exercised command in a deliberately understated fashion. An English gentleman, he went to war in civilian clothes and rejected the heroics of Alexander. Even so, he frequently exposed himself to danger, but not out of a desire to impress his troops but because short-range weapons, narrow frontages, and limited visibility required his presence in and along the battle line. Fifty years later, Grant, commanding a populist army in which, as Keegan points out, compliance with orders rested more on consent than on sanctions or heroics, took pains to appear as a common man. Given the much enlarged scale of combat, together with the greater range and lethality of weapons, Grant normally directed his forces from the rear edge of the battle zone. And, finally, Hitler, the "false hero," commanded his armies from headquarters well to the rear, from where, especially after 1941 when he appointed himself commander in chief of the army as well as of the armed forces, he repeatedly committed his forces to operations demanding the impossible. Throughout the war Hitler avoided personal contact with his troops, while maintaining his false mask of command by having his propaganda machine proclaim that, as a former combat soldier, which indeed he was, he understood and shared the ordeal of his men.

In his conclusion Keegan sums up and compares the various commanders and, moving away from history, makes a special plea. The world, he asserts, is now in the "post-heroic" stage where nuclear weapons not only have forced unification of political and military leadership but where, given the ultimate risks, defensive thinking rather than the search for victory by offensive means should be stressed.

Keegan's approach, reminiscent in some ways of Carl G. Jung's theories, may not appeal to all specialists, and some of his bold judgments might be disputable. But this is history in the grand style, both magisterial and well written. Of course some chapters are better than others. The treatment of

Wellington is superb, and the study of Grant gave me new insights. The essay on Alexander also is rewarding, but I had misgivings about the treatment of Hitler. Too much, I thought, was made of his experiences as a battalion runner in World War I and of his success in overawing and controlling his professional senior officers, and too little attention was given to the underlying dynamics of the Prusso-German military tradition and the National Socialist regime. But these are quibbles that cannot detract from the very real value of a major work that should be read, digested, and debated.

GUNTHER E. ROTHENBERG
Purdue University

RICHARD HOUGH. *The Longest Battle: The War at Sea, 1939-45*. London: Weidenfeld and Nicolson. 1986. Pp. xi, 371. £14.95.

Richard Hough has written over the past thirty years a sizable number of popular histories, generally on naval subjects. He now makes a brave attempt to present an account of the naval part of World War II, which he considers "the longest and greatest battle of all time, extending to every ocean and sea" (p. 1). Hough aims "to present this non-stop battle from a sailor's view and in terms of personal experience" (p. 1). Fortunately, Hough's definition of sailor ranges from admiral to stoker, so there is sufficient scope for some discussion of strategy. The technique is to bolster the narrative and analysis of events with lengthy quotations from memoirs and interviews, frequently from obscure or long-forgotten works. The interviews are varied and include Lord Mountbatten, the subject of a biography by Hough, and others with interesting experience of the war, although there does not seem to be any pattern as to how they were chosen. The book concludes with appendixes of which the most useful are capsule biographies of some two dozen of the leading admirals. The bibliography, on the other hand, titled "Some Printed Works Consulted," is less than a page and, considering the subject, is laughable.

Hough begins well. His accounts of subjects such as the submarine war in the Atlantic, the battles in the Mediterranean, and the destruction of the *Bismarck* are generally effective. Hough does not limit himself to a predominantly British point of view. The American experience at Midway and the campaigns centering on Guadalcanal receive full treatment. The eyewitness narratives he includes are usually well chosen, interesting, and, it would be no exaggeration to say, frequently gripping. Those with no personal memories of the war or new to the subject and students with an interest in naval and military affairs will find this

an enjoyable book to read. Unfortunately, the strategic explanation can become lost in the operational narrative. Hough can be effective in explaining what and why things happened, but this is not always the case. Balance is another problem. Hough hardly mentions, for example, the strategic debate over the Norwegian campaign—the whole question of Operation HAMMER (the aborted project to strike the Germans at Trondheim)—but has a lengthy account of the sinking of the carrier *Glorious*. The attack on the French fleet at Mers-el-Kébir is mentioned but not the expedition to Dakar or the final fate of the French fleet at Toulon. The naval aspects of the North African landings, the invasions of Sicily and southern Italy, and the long ordeal off Anzio are also omitted. On the other hand, we get a long account of an obscure action by coastal forces in the Adriatic. Is this exciting? Yes, but it is hardly a balanced treatment.

Hough's other problem is that he runs out of space. His most effective account of the war really ends with the year 1942. The problem again is one of balance. The pursuit of the *Bismarck* extends across a good portion of two chapters (with a two-page map); the American submarine campaign against Japan, which Hough admits is important, receives barely six pages. The great air-sea battles of Leyte Gulf also receive short shrift compared to what happened before. Iwo Jima, Okinawa, and the kamikaze attacks are hardly mentioned. Hough considers the war to have been essentially decided before Leyte, but none of the American or British seamen under kamikaze attack off Okinawa would have considered the war over, and if Hough's stated objective was to recount the experience of the war then he has failed by his own standards. The problem may be his publishers. When will the cost accountants who seem to be running the presses these days realize that a writer with a big job to do ought to have the space to do it properly? At 341 pages of text, another 100 pages would not have resulted in an excessively long work. On the other hand, just as every warship was a compromise between gun power, defensive armor, and speed, Hough, if faced by constraints, should have compromised to produce a balanced design. He has not done so, and his book is far less effective than it might have been.

PAUL G. HALPERN
Florida State University

DITTMAR DAHLMANN. *Land und Freiheit: Machnovščina und Zapatismo als Beispiele agrarrevolutionärer Bewegungen*. (Studien zur modernen Geschichte, number 35.) Stuttgart: Steiner. 1986. Pp. 296. DM 68.

In the shadow of great national sociopolitical upheavals, there frequently also occur revolts of a limited geographic and social nature. This was the case in Russia in 1917–21, where Nestor Makhno led a peasant revolt in the southern Ukraine, and in Mexico, in the state of Morelos, where the peasantry rose in 1911–19 under the original leadership of Emiliano Zapata. Dittmar Dahlmann treats both of these as instances of "agrarian revolutionary" movements in opposition to the recent and rapid development of "capitalist" relationships in the countryside at a time when the central political and social institutions had largely collapsed. Both peasant revolts (or revolutions) have been extensively studied and have generated a vast primary and secondary literature that Dahlmann has conscientiously examined, as his notes and extensive bibliography show. He has added a few small details culled from foreign government archives or unpublished memoirs.

The book gives a workmanlike summary of the events and ideological (or programmatic) stands taken by the peasants in revolt. The biographical-psychological sketch of Makhno (and to a lesser extent of Zapata) brings out the tensions between the intellectual influences (anarchism, for example, in the case of Makhno) on the leaders and the behavioral norms and traditions of the followers. But Dahlmann is too unsure of the historical background and contemporary context to do justice to the complexities of the situation in each case. Like so many of his West German colleagues, he is mesmerized by irrelevant or artificial sociological models and uncritically resorts to a Marxist vocabulary whose relevance is very doubtful. On the basis of his own evidence, he is unable to establish convincing causal or functional relationships between his sociological models, economic premises, historical assumptions, and the peasant movements he tries to account for. His statements about the peasants' yearnings to reestablish (or preserve) a traditional past in protest against new capitalist market relations are not supported by concrete evidence, and his efforts at schematizing the socioeconomic differences among the peasantry end in a complete muddle. And how can one, for example, explain the Makhnovist movement without reference to the impact of World War I on Russia, the Ukraine, and the peasantry or see the revolution and Civil War in such superficial and simplistic terms?

Since most Americans are not too familiar with the German language, they will turn to the many works in English that have been written on these two revolts; those, however, who will take the trouble of reading this clumsy and repetitious monograph will not find much enlightenment.

MARC RAEFF
Columbia University

R. C. MICHIE. *The London and New York Stock Exchanges, 1850–1914*. Boston: Allen and Unwin. 1987. Pp. xv, 312. \$49.95.

This fascinating study is comparative economic history at its best. Rather than concentrate solely on the internal workings of the London and New York Stock Exchanges, R. C. Michie deals with their “external” relationships. Thus, the book starts with a discussion of the London Stock Exchange and its associations with provincial and foreign exchanges. Michie covers the impact of the telegraph, cable, and telephone in integrating domestic and international securities markets. He considers both the domestic and international role of the London Stock Exchange, as well as the exchange and capital markets and money markets. Michie is excellent on the institutional arrangements.

Michie challenges—at least as far as the generalizations have been applied to the London Stock Exchange—the substantial literature that argues that London as a financial center failed in domestic finance from 1850 to 1914. He maintains that the need to raise capital was rarely the reason for the joint-stock company form or for a London Stock Exchange quotation. Rather, the reasons for the joint-stock company form lay in sharing risk, stimulating “custom,” “convenience” (it was easier to manage), continuity, and ease in mergers. Companies that obtained a London Stock Exchange quotation were existing businesses, rather than new ones “seeking to raise finance.” Nonetheless, the presence of the London Stock Exchange (as a secondary market) made new issues attractive. Michie concludes that for governments and for costly infrastructure informal markets were inadequate, and the London Stock Exchange served these needs very well, albeit in an indirect manner. He is convinced that the exchange was an effective allocator of the nation’s savings and met the demands made on it.

The liquidity created by the London Stock Market blurred the distinction between short- and long-term capital, at least to the financial intermediary (but not to the long-term borrower). Michie shows how banks’ short-term funds could be employed to hold long-term securities on a temporary basis.

The London Stock Exchange was a financial entrepôt for the rest of the world. By 1913 more foreign securities were quoted than domestic ones. Michie makes the extraordinary, at least to me, statement that the proportion of funds employed on the London Stock Exchange could be in the range of two-thirds foreign, one-third domestic (p. 150). There is, however, no doubt that London

was the world’s international financial center *par excellence*.

By contrast the New York Stock Exchange before World War I was mainly a market for domestic securities. Michie discusses the New York Stock Exchange’s relationship with other domestic stock markets, inside and outside New York City. He comments on transatlantic arbitrage (in American securities). The New York Stock Exchange’s role in creating liquidity for financial institutions is well explained. His last chapter compares the New York Stock Exchange with the London one vis-à-vis the ownership of the building, membership rules, time for settlement, type of securities (domestic or foreign) quoted, criteria for quotation, and so forth. Michie tries to explain why there were differences, along with the resulting consequences.

The value of comparative history is that it poses new lines of inquiry. Students of American economic history will undoubtedly have reservations about some of Michie’s interpretations (as will students of British economic history), but all will appreciate how fruitful such a comparative approach can be. I highly recommend this study to historians who specialize in modern economic and business history.

MIRA WILKINS

Florida International University

CHARLES BLINDERMAN. *The Piltdown Inquest*. Buffalo, N.Y.: Prometheus Books. Pp. 261. \$22.95.

In the history of science, and, indeed, of culture, one area above all others, perhaps, offers us a clear field for arguing the social construction of the intellect and of the disciplines into which we mold it: the issue of forgery. The motives that lie behind the world’s most famous forgeries—from the faked antiquity of manuscripts to the endless sightings of long-necked monsters in the waters of Loch Ness—suggest to us that what is at stake in establishing an intellectual claim, in determining one’s territory on the map of scholarly imperialism, is more than the disinterested pursuit of knowledge. In his study of the notorious Piltdown Man hoax, Charles Blinderman alludes to but never fully explores this aspect of his tale, preferring instead to play detective in a bewildering host of literary styles.

Blinderman clearly has a thorough and impressive grasp of his complex and confusing subject matter, but, in attempting to act as historian, archaeologist, detective, judge, jury, and social commentator all at once, the book frustrates more than it enlightens or entertains. While acknowledging the problems of assessing the potential

perpetrator of the forgery, his own position is unequivocal. The basis of the book is, topically, to assert the legitimacy of the scientific method and of rationality and evolution as its partner against the encroachment of creationist fundamentalism.

It is an indecisive book, not in establishing the identity of the forger but in failing to sustain any single and consistent voice. Blinderman begins his book with the voice of the paleontologist and archaeologist outlining the technical as well as cultural significance of the Piltdown finds. The prose is irritatingly flippant, larded with irrelevancies (for instance, the Lady Bracknell passage [pp. 11–12]), but impressive nonetheless in its clarity. He moves from this to a historical chronology of the fortunes of Piltdown Man before turning to his consideration of the suspects. This section of the narrative, comprising nine chapters in all, is in the least successful of Blinderman's many voices. It is overlong and indulgent, jumping from legalistic prose to parodic fictionalizing, none of it executed with much style.

It is really only in his final chapter that Blinderman's most convincing and satisfactory voice is allowed to emerge and only here that he even begins to acknowledge the cultural construction and contentious implications of his subject matter. Although the dearth of footnoting signals his laudable intention to widen the net of his readership beyond the academic institution, he might still have fruitfully probed the demystification of science as cultural construct a little deeper. He opts, rather, to use Piltdown Man as a vehicle for his championship of evolutionary science. Half-way through the book he discusses the proposition that the hoaxers may have been prompted to use "fraud in the service of truth"; that in the end is his story, and his summing up is broadly optimistic about the future of science as an intellectual discipline.

Finally, and in the interests of accuracy, Blinderman should take note that neither in earlier times nor, indeed, in modern London would a dish of Moo Goo Gai Pan be followed by "cookies of fortune." They are, I am assured by my American students, an invention of San Francisco's Chinese community and are known in Britain only in the pages of the modern American novel.

PHILIPPA LEVINE
Florida State University

continental invasions to explain change, has been abandoned. In its place the long-established concerns with economic activity have been joined by more refined ideas about social processes with which to offer explanations for the material. And, while these intellectual changes have taken place, increased funding for rescue archaeology and additional programs of field survey and research have yielded ever increasing quantities of data.

Given this explosion in both material and ideas, the need for a lucid, overall guide to our current understanding of the material has become urgent. Timothy Darvill's book is a welcome attempt to introduce this material to a wider audience. Well-illustrated, this volume takes the reader through the entire prehistory of England, Wales, and Scotland, from the earliest hunter-gatherer settlement to the Roman invasion. The narrative is organized around what Darvill identifies as the major periods of social change and explores, at a breathtaking pace, the material that defines each of these periods. So much ground is covered that the descriptions can never be more than schematic; the reader is invited to follow up this initial encounter through a well-organized bibliography.

What is gained and what is lost in such a wide-ranging survey? Undoubtedly, the gain is that we now have an up-to-date general synthesis of British prehistory that can be put into the hands of students and others beginning an interest in this period in the knowledge that their interest will not be killed by the mindless cataloguing of material. But Britain offers a remarkably diverse set of environments over which to trace the history of human settlement. That diversity all but disappears in Darvill's account, from which emerges a broad uniformity of historical processes. Regional patterns are referred to, but, with the exception of Roman military and diplomatic intervention in southern Britain, it is difficult to understand what produced them. This lack in the regional perception of much of the material is compounded by a lack of detailed maps by which the reader may locate even the more significant sites that are discussed in the text. Nonetheless, this is an attractive book that will bring new ideas and material before a wider public.

JOHN C. BARRETT
University of Glasgow

ANCIENT

TIMOTHY DARVILL. *Prehistoric Britain*. New Haven: Yale University Press. 1987. Pp. 223. \$25.00.

Much has changed in our understanding of British prehistory in the last thirty years. The analysis of cultural groupings, and with it the emphasis on

JOHN BOARDMAN *et al.*, editors. *The Oxford History of the Classical World*. New York: Oxford University Press. 1986. Pp. vii, 882. \$39.95.

In 1980, when Michael Kammen edited for the American Historical Association a collection of

studies entitled *The Past before Us: Contemporary Historical Writing in the United States*, conceived as "a report to historians all over the world" to elucidate the "panoramic range of the interests, activities, problems, and achievements of historians in the United States," it began with a chapter on "American Medievalism." The dismissal of ancient history came as a surprise to American (and Canadian) ancient historians, as well as to our colleagues in Britain and on the Continent, who have been laboring diligently in Clio's vineyard with contemporary methodologies and perspectives. Moreover, the flyleaf quoted Henry Adams's injunction to the association that history "shall include all races, all countries, and all times." Scholarship on the classical world includes eminent contributors in Britain, and in North America there is a thriving Association of Ancient Historians with a periodical, *The Journal of Ancient History*. And, recently, the dean of ancient historians here, Chester G. Starr, has discussed for the profession the ongoing methodological dialogues and the advances and challenges in the scholarship of ancient history in *Past and Future in Ancient History* (1987).

Now our colleagues in Britain have given us this elegant, authoritative, monumental, eminently readable one-volume survey of Greek and Roman civilizations that will take its place as a new standard companion to the ancient world for the general public. Needless to say, it serves a purpose far different from that of the multivolume *Cambridge Ancient History*, which is undergoing an updated revision. The time is indeed ripe for the volume under review here. Witness the extraordinary differences in conception and content between Oxford's *The Legacy of Greece*, edited by Richard W. Livingstone in 1921, and *The Legacy of Greece: A New Appraisal*, edited by M. I. Finley in 1981.

The aim of this new *Oxford History of the Classical World* is to make available an "accessible narrative" for the general public of nonspecialists (and one should add college students) embodying the latest advances in scholarship on the history and achievements of the Greeks and Romans. In thirty-three chapters by thirty contributors (including editors John Boardman, Jasper Griffin, and Oswyn Murray), all distinguished British scholars, the volume ranges from the emergence of the Greek polis and its civic institutions to the waning of urban civilization in the late Roman Empire, covering over one thousand years. Between Griffin's charming introduction and Henry Chadwick's rather compressed envoi, the various contributors cover, with sparkling felicity, wit, and clarity, most of the significant aspects of classical antiquity. True, there are lapses here and there—

the riding of an academic hobby horse with overspecialized details or occasional erring in the direction of overgeneralized treatment. The areas especially emphasized are political history and institutions, literature, the visual arts, philosophy, and religion. For me the following chapters were the most attractive: Oliver Taplin's "Homer," Martin West's "Early Greek Philosophy," Peter Levi's "Greek Drama," Robert Parker's "Greek Religion," Elizabeth Rawson's "The Expansion of Rome," R. O. A. M. Lyne's "Augustan Poetry and Society," and Andrew Lintott's "Roman Historians."

It will surprise many that Mycenaean Greek origins are given no place in this history, nor are Etruscan culture and life in the leading Roman provinces. And economic history is relatively scantied. (It is true that few ancient historians are equipped with economic theory and methodology.) Other than Murray's treatment of Athenian women, there is little on women in antiquity, despite the avalanche of sophisticated studies in the last fifteen years.

Indeed, Clio's first protégé, ancient history, has moved in the last few decades from the relative isolation of the specialist's preserve into the mainstream of world history in its perspectives and methodologies, despite its random and fragmentary evidence and the lack of credible quantitative data. In recent decades we have had a steady accrual of new inscriptional, papyrological, and archaeological evidence, which has answered many questions and posed new ones. Recent critical scholarship also has contributed to the reassessment of the classical world in this masterly work by scholars who have at their command the distillation of contributions in thousands of specialized articles and hundreds of books.

For the general reader the volume is equipped with excellent chronological tables, which correlate political events with cultural developments. Each chapter has a coda with judiciously chosen books for further reading. The index is skimpy for such an array of themes but perhaps adequate for the general reader. The enormous number of illustrations add instructional value to the text (even if eight of the black-and-white ones are washed-out blurs).

Such a book as this one has special value at a time when we anticipate (in 1993–94) the 2,500th anniversary of the establishment of the Athenian democracy. One might have wished for a concluding chapter on the legacy of the classical world, even if only on the fascination of Europe in the eighteenth and nineteenth centuries with Hellenism, especially German Neohellenism and British Grecomania in the Victorian and Edwardian peri-

ods, as well as the veneration of the Roman Republic in the American revolutionary age.

Shelley could write "We are all Greeks," whereas W. H. Auden, in 1955, perceptively cautioned us on the alien character of Greek culture, on the "unlikeness of the Greeks to ourselves." R. R. Bulgar reminded us (in *The Legacy of Greece* [1981]) that "for our generation Greece [one should add Rome] wore a different face, and there is no reason to suppose that we have come to the end of its potential metamorphoses."

For our time the volume under review is a scintillating distillation of the wisdom we now have on classical civilization, as illuminating for the public at large as it is for college students of Greek and Roman culture.

MEYER REINHOLD
Boston University

WERNER HUSS. *Geschichte der Karthager*. (Handbuch der Altertumswissenschaft, part 3, number 8.) Munich: C. H. Beck. 1985. Pp. xii. 578. DM 176.

This is the most comprehensive and fully documented history of Carthage to appear since S. Gsell's monumental eight-volume *Histoire ancienne de l'Afrique du Nord* (1913–28). Thorough and critical in his collection and evaluation of the literary sources, abreast of the most recent archaeological, epigraphical, and numismatic research, and exhaustive in his citation of the secondary literature, Werner Huss has written what should remain the standard political history of Punic Carthage.

The book adheres to the traditional format of the *Handbuch der Altertumswissenschaft* series. It is an encyclopedic work of reference rather than a book to be read for pleasure. Each chapter begins with a list of primary sources and major secondary treatments. Every major and minor problem of Carthaginian history is discussed with citation of all relevant literature. A brief introductory sketch of the history of Carthaginian studies is followed by excellent chapters on the character of Phoenician colonization, the founding of Carthage, the topography of the city, and the ethnic character and qualities of the Carthaginians. The bulk of the book is given over to a detailed narrative of Carthaginian political and military history down until the Roman destruction of the city in 146 B.C. Huss concludes his history of Punic Carthage with a series of brief topical chapters on the constitution and administration of Carthage and its empire and on Carthaginian military science, economic and social life, literature and art, and religion. An epilogue outlines in the briefest of terms the history of Carthage under the Romans.

The sources present a major problem for the historian of Carthage. Not only are they sparse but also they are written almost entirely from the point of view of Carthage's Greek and Roman enemies. While maintaining his objectivity, Huss has sought to redress the balance as much as possible. The Punic Wars appear as the First, Second, and Third "Roman Wars." For Huss Rome was the aggressor in all three wars. In 264 B.C. economic motives—greed for the riches of Sicily—led Rome to force a war with Carthage. In 218 B.C. war was the result of Rome's desire for territorial expansion in Spain and the determination to destroy Carthage as a great power. Carthage's final death agony was a calculated act arising out of the fear—not unjustified from the perspective of the Roman Senate—of the reemergence of Carthaginian economic and military power. Not everyone will accept his conclusions, but every student of ancient history will find Huss's reconstruction of Carthaginian political history meticulous and challenging.

Of the topical chapters, those on the Carthaginian constitution, which excited the admiration of Aristotle, and on the religion of Carthage, which excited the disgust of Greeks and Romans alike, are the fullest and most valuable. On the whole, however, given the excellence of Huss's narrative chapters, his treatment of other aspects of Carthaginian life will seem disappointing. In a book of this length, we might expect more than a seven-page discussion of Carthaginian social history. The history of the family is relegated to a short paragraph; anyone interested in the status of women must rest content with the notice that "the wife seems to have had a respected position." The chapter on art is so brief as to be of little value. There are no plates, and the text is a mere skeleton on which to hang bibliographical references.

The very brevity of these chapters points to the need for a more comprehensive social and cultural history of Carthage. Such a work will need to consider in detail the material from other Punic cities and settlements in North Africa and throughout the western Mediterranean. It will need to give due attention to the period of Roman domination. A history of Carthage can end in 146 B.C. A history of the Carthaginians can not, and the continuation and transformation of Punic culture under the Romans offer a rich vein for scholarly exploration.

The study of Carthaginian society and culture requires a willingness to integrate the archaeological and literary sources. From a methodological point of view, this is a weakness of Huss's treatment. He knows the archaeological evidence, but on occasion he fails to emphasize its importance or see its relevance for his argument. Three instances

will suffice. The archaeological evidence should form the basis for any discussion of the date of the foundation of Carthage. Huss treats it rather as an appendix to his lengthy examination of the ancient literary evidence (p. 43). Huss comments on the importance of the excavations at Pyrgoi as evidence for Carthaginian influence in central Italy at the end of the sixth century (p. 66), but he makes no mention of the possible bearing of that fact on the question of the date of the earliest treaty between Rome and Carthage. Finally, in commenting on the sacrifice of children in Carthaginian religion, Huss refers to the recent excavations at the Tophet and then goes on to write: "As the religion of Carthage increasingly came under humanizing influence—a process in full swing by the sixth century at the latest—many Carthaginians came to see human sacrifice as wrong" (p. 539). In point of fact, the excavations at Tophet make it amply clear that "precisely in the fourth and third centuries B.C., when Carthage had attained the height of urbanity, child sacrifice flourished as never before" (Lawrence Stager and Samuel Wolff, "Child Sacrifice at Carthage: Religious Rite or Population Control," *Biblical Archaeology Review* 10 [1984]:42).

However one may disagree with the author on details, this book is a major scholarly achievement, which puts every student of the ancient world in Huss's debt.

J. RUFUS FEARS
Boston University

RONALD SYME. *The Augustan Aristocracy*. New York: Clarendon Press of Oxford University Press. 1986. Pp. viii, 504.

A review of a book by the preeminent living Roman historian risks appearing facile or presumptuous. (The following may achieve both.) The text of the book was completed in 1982, with some chapters antedating 1976, but the extensive genealogies postponed publication several years. The book comprises thirty chapters: one on the "nobilitas," followed by six narrating Augustus's rule and his shifting relationships with families of the high aristocracy, then twenty-two chapters devoted largely to the reconstruction of the histories of those noble families from the late Republic through the Julio-Claudian era, and the final chapter, offering an "apologia for the Principate."

The core chapters, described in the preface "as a kind of incomplete work of reference," recount the lives, careers, and matrimonial alliances of Augustan senators, focusing on the families of those who reached the consulship during the decade of "Monarchy and Concord" (16–7 B.C.).

The task is like piecing together a puzzle of many thousands of pieces, most of which are missing. The detailed knowledge and ingenuity in this partial reconstruction are awesome. Ronald Syme's talents here are unmatched, as suggested by the footnotes, which show him largely in dialogue with his own past work and the *Prosopographia Imperii Romani*. Anyone commenting on the life or career of Augustan aristocrats will need to consult the book for revisions of standard views in order to avoid the sin of "inadvertence."

But this book is much more than a prosopographical catalogue, for it traces the general theme of how the ancient noble families came "into nexus with the dynasty," prospered for a time, and then went down: "'Saeva pax' was more deadly than civil war" (p. 243). The theme is compelling, not only because of the mass of prosopographical detail but also because of Syme's exquisite sensitivity to the political culture of the Augustan age (see, for example, the subtle remarks on the significance of ordering of names [p. 75]).

Although the book is a masterpiece, its polemic draws attention to its limitations as social history. Advertising his inability "to emulate the easy people 'who write without fear or research,'" Syme insists that "though the masses may count for weight and pressure in the revolutionary wars through the resources of the provinces and the demands of the troops . . . the writing of history does not accord well with bare abstractions or with appeal to the voiceless and anonymous . . . Those who in any age compose the annals of Rome are moved towards 'clarorum virorum facta moresque.' They run a risk of dispraisal from adepts of recent fashions and doctrines, being condemned for prejudice or a narrow outlook" (p. 13).

As the pendulum of "fashions" swings back in 1989, this manifesto demands scrutiny. As Syme recognizes, his theme necessarily involves questions of age at marriage, fertility, and mortality—in other words, the subjects of demography, which must be dealt with at the level of patterns and probabilities, rather than the individual. On these subjects, Syme's Tacitean style and analysis can be unilluminating, as when infant mortality is discussed in terms of children who "had aggregated to the nameless nations of the dead" (p. 20). For these subjects K. Hopkins's approach in *Death and Renewal* (1983) is to be preferred. Ideally, future work on the reproduction and replacement of the Roman aristocracy will combine a modern sophistication in demography with Syme's remarkable knowledge of individuals and naming practices. In such an endeavor, this book will be an indispensable starting point.

RICHARD SALLER
University of Chicago

MEDIEVAL

PETER GODMAN. *Poets and Emperors: Frankish Politics and Carolingian Poetry*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xviii, 199. \$45.00.

After a hiatus of two centuries, during which no poet had sung to that note, secular panegyric reappeared in the court of Charlemagne. The genre continued to draw authors during the rest of the Carolingian age. Neither the fact of this reemergence nor its character and significance had received the attention they deserved prior to the publication of the volume under review. Displaying a scholarship as discriminating as it is broad and deep, an unusual felicity and accuracy as a translator, and an unfailing lucidity of style, Peter Godman makes three main points about Carolingian panegyric. First, it was anything but homogeneous. The literary focus and strategy changed from reign to reign and from poet to poet. Second, panegyric sheds considerable light on Carolingian political and cultural history. And, third, the encomiasts appealed to a diverse group of sources as they commented on current events and on their own situations.

Following a helpful introductory chapter on Venantius Fortunatus as a transition figure between late Latin and Carolingian panegyric—a chapter that, aside from its relevance to his larger theme, provides a fresh and cogent reading of Venantius himself—Godman gives us three chapters presenting the Carolingian political poets in chronological order. He concludes by underlining the features of the genre that flowed into the Ottonian age. In Charlemagne's reign, he shows, poets such as Alcuin and Paul the Deacon renewed the secular encomium by praising the ruler as a *rex doctus* and by annexing that *topos* to sacred kingship and defense of the faith. In the 790s these religious and literary ideas were fused by Angilbert in the image of Charlemagne as the new David. With his imperial coronation in 800, a number of poets shifted to the invocation of Charlemagne as the new Augustus and turned to Vergil and other golden age poets as their chief models. Louis the Pious and his successors posed more of a challenge to encomiasts, especially those trying to second guess which claimant to the throne would succeed. Theodulph reflects the dissension among the heirs of Charlemagne. His appeal to the Ovid of the *Tristia* as a model suggests how that poet of exile could inform an author who found himself banished for backing the wrong horse. On the other hand, Sedulius could praise different contestants generically, and interchangeably, working as he did from the se-

cure base of episcopal patronage, which was growing more important in the mid-ninth century. In the reign of Charles the Bald, John Scottus added a Hellenic twist to the *rex doctus* theme, no doubt out of self-interest, and Hieric of Auxerre expanded it, recasting Charles as a Platonic philosopher-king.

These highlights, brief as they are, suggest the richness and pertinence of Godman's findings, for political and literary history alike. As he shows so successfully, Carolingian encomium is a valuable source for tracking both contemporary political events and political ideas. With respect to the classical tradition, and this is equally important, Godman strengthens the case for a Carolingian renaissance that did more than merely salvage and disseminate the classics, untouched by human hands. For, as he demonstrates, the political poets drew on the classical legacy with independence and creativity, reshaping the literary form of panegyric itself and making their own individual choices among their classical models. For all these reasons, this book stands as a major, and welcome, contribution to Carolingian studies, which should be required reading for any student of that period.

MARCIA L. COLISH
Oberlin College

MARCO MOSTERT. *The Political Theology of Abbo of Fleury: A Study of the Ideas about Society and Law of the Tenth-Century Monastic Reform Movement*. (Middel-eeuwse Studies en Bronnen, number 2.) Hilversum, The Netherlands: Verloren. 1987. Pp. 224, f. 45.00.

Marco Mostert's study of the political theology of Abbo of Fleury is a modestly useful contribution to the literature concerning tenth-century monastic reform. At first glance, a study of the reformer's ideas about society and law seems to promise the reader a unique window through which one might view the interplay of the medieval monastic mindset and the social realities of tenth-century France. On closer examination, this volume provides a much narrower, and much less exciting, vista. Mostert's book is a largely mechanical study of the political and legal vocabulary employed by Abbo of Fleury. Mostert's methodological assumption is that a study of the legal and political terminology used by Abbo is his diverse writings, especially the *Passio Sancti Eadmundi*, the *Liber Apologeticus*, the *Collectio canonum*, and Abbo's letters, would yield "an imaginary text" (p. 19), comprising Abbo's political theology. Mostert's book professes to be a commentary on this imaginary political and social treatise.

There are serious problems with Mostert's assumption that the political, legal, and social terminology employed by an author throughout a heterogeneous medley of writings amounts to the same thing as the political, legal, and social ideas of the same author, arrayed in a single, coherent exposition of his "political theology." Context counts, and the context in which Abbo of Fleury employed his political vocabulary was the fragmented, unfocused range of his occasional writings, composed for specific and transient purposes.

As a result, Mostert's study struggles to rise above the level of a mere glossary of Abbo's vocabulary. The introductory chapters, which detail Abbo's career from ca. 940 to 1004, point out that Abbo's early life as a monastic schoolteacher and librarian sculpted the ideas and concerns of the mature, politically active reforming abbot. But, despite the efforts to place Abbo in a broader context, Mostert's portrait of the abbot is unidimensional. Mostert's narrow vision of Abbo's "political theology" results from one of two contingencies. Either Abbo himself failed to consider society, law, and politics in light of any issue beyond the immediate struggle for monastic exemption from episcopal control, or else Mostert fails to connect Abbo's use of political or theological terms to any context save the most obvious and pressing of the abbot's social perceptions and political concerns. One suspects that a more sensitive reading of Abbo's works might have produced a richer yield.

Despite these fundamental limitations, Mostert's study makes a useful contribution to our understanding of late tenth-century social, political, and legal ideas by illustrating how the tenth-century monastic reformers adapted conventional medieval notions about clerical and lay society, kingship both ideal and practical, and authority within the church. Unfortunately, even at this level, the book is slightly marred by the Dutch author's difficulty expressing himself in English. For example Mostert refers to the "siege" of Cahors (p. 60) meaning the see of Cahors, and he quotes Abbo's discussion of a papal privilege "which I, though unworthy, have earned to receive" (p. 58). Careful editing might have significantly improved the presentation.

RICHARD M. FRAHER
School of Law
Indiana University

JEAN-PIERRE DEVROEY. *Le polyptyque et les listes de biens de l'abbaye Saint-Pierre de Lobbes (IX^e-XI^e siècles)*. Brussels: Palais des Académies. 1986. Pp. cxxvi, 84.

Jean-Pierre Devroey, author of an earlier critical edition, *Le polyptyque et les listes de cens de l'abbaye de Saint-Remi de Reims* (1984), presents here an introduction (pp. xviii-cxxvi) and revised edition of the previously published Lobbes documents. The edited texts (pp. 3-58) are followed by an exhaustive table of names of persons and places and a useful index of technical terms.

At issue are three documents now surviving only in eighteenth-century copies: a polyptyque called the *descriptio villarum* and two lists of monastic property holdings. In a well-argued, revisionist dating schema, Devroey reasons that the *descriptio villarum* is not the fragment of a larger polyptyque but rather, in a first section of the present manuscript, an inventory of the properties attached to the conventual *mensa* in 868-69. Another section contains what is, in essence, a second polyptyque, related to the functions of custodian of the church, doorkeeper, and hospitaller and dated to 889, shortly after the division of the monastic patrimony between the monks of Lobbes and the bishop of Liège, who assumed administration of the abbey following a decision by Arnoul of Carinthia. Interpolations of these documents took place in the years 960-65; the list of 889 was revised and amplified again in the early eleventh century.

The short list of possessions, the "liste courte" in the author's terminology, is a simple enumeration of properties, including 137 *villae*, dated to the 889 division, with later interpolations in the domains of certain *pagi*. The "liste longue" (late tenth century to 1038) is based on the "liste courte," but with the addition of 46 *villae* and certain toponymic modernizations as well as modifications in the order of the list. Demographic growth with new centers of population and the expansion of temporal holdings accounted for the evolution in monastic patrimony.

Devroey sees at Lobbes in 868-69 the concrete realization of Carolingian orders for the inventorying of monastic properties. The reserve was described without mention of courtyard buildings (*curtis*) but with the extent of lands and meadows in *bunuarua*. The woods were measured in pasturing potential of pigs, the vineyards in *modii* of wine produced. The peasant tenures (*mansi*), 527 in all, fell into the categories of 69.6 percent without label, 26.9 percent *ingenuiles*, 2.7 percent *serviles*, with one *villa* having *mansi lidorum*. Devroey reasons that the servile tenures had to be listed since they were exempt from military service. The most startling lacuna concerns the inhabitants. Only the *haistaldi*, a marginal group of peasants, were mentioned in any detail, thereby eliminating the possibility of controversial demographic studies of inhabitants of the Lobbes estates. Dues owed in

cereals were particularly heavy in comparison with the data of other polyptyques, and there was only an occasional levy in wood or artisanal production. Overall, payments in kind and in money, in response to the daily needs of the monks, were far more important than labor service. The exploitation of the reserve was probably facilitated by a potential labor pool of peasants called *sessi* with very small tenurial holdings, mentioned briefly in the polyptyque of 889. Although emphasizing the ways in which the Lobbes estates differed from the ninth-century norm, given the diversity of regimes of exploitation among the Lobbes holdings, Devroey rejects the regional ground rent thesis proposed recently by L. Kuchenbuch for the area between the Meuse, the Escaut, and the lower Rhine.

On balance this study is a meticulous reexamination of known sources providing greater precision of dating and interpretation. The multiple geographic sketches and lists of properties that accompany Devroey's discussion are generally useful, although at times elements of the keys and capitalization systems are not sufficiently defined. One can only hope that other scholars will have the patience and courage to apply modern editing and analytic techniques to the admirable, but flawed, text editions of earlier decades.

KATHRYN L. REYERSON
University of Minnesota

MICHEL AUBRUN. *La paroisse en France des origines au XVe siècle*. Paris: Picard. 1986. Pp. 269. 240 fr.

Michel Aubrun has marshaled an array of evidence for this consideration of the parish in medieval France. The evidence of the lives of saints, of Carolingian legislators, of the Latin Fathers, and of the acts of councils are all cited *in extenso*, along with the materials concerning the parish (and, in particular, its relationship to the monastic and episcopal church) that he has discovered in the documents of practice—the medieval charters. Most of the material presented here is not new but is gleaned from the work of a variety of important regional studies of the church, that of Elisabeth Magnou-Nortier for Languedoc and of Jean Becquet, Joseph Avril, André Chédeville, Guy Devailly, and Aubrun himself for the north, among many others.

The volume contains a wealth of extremely interesting details about parish life found elsewhere only in scattered footnotes or in the proceedings of very specialized conferences. One learns, for instance, of the importance of funeral services and burials as sources of revenue for the parochial clergy and of the consequent disputes

that arose when funerals were conducted in monastic or mendicant churches instead. Cemeteries were important as places of asylum in which markets and fairs were held and which sometimes became centers of settlement in the disturbed times of the eleventh century. Similarly, Aubrun shows that, despite the fact that fortification may have resulted from the insecurities of the period, fortified churches were condemned by church councils as liable to be havens for criminals and brigands—and therefore not protected by the peace movement. From the viewpoint of the parish, most religious communities were parasitical and intrusive. According to Aubrun, only the Grandmontines appear to have taken a reasonable position regarding the parish.

There are weaknesses in Aubrun's presentation, particularly with regard to women, who are given no other consideration than as scandalous wives of priests or as particularly disreputable simoniacal owners of tithes. The Cistercian position on tithes (and its revision in 1215) is also misunderstood in its consequences for the support of parish churches. Although setting out to describe the parish in all its aspects, this volume has more to say about clerical personnel, their relationship to the bishop and to patrons (including monastic and collegial churches), and the revenues that supported them than about parishioners themselves; this, however, is the bias of the surviving documents.

Particularly interesting is the collection of supporting maps and Latin documents translated into French that accompanies the text. Many of these would be suitable for classroom use. Unfortunate are the consultation only of works in French and the incomplete nature of certain of the footnotes, which makes it difficult to follow up some of the most intriguing information.

CONSTANCE H. BERMAN
University of Iowa

R. ALLEN BROWN. *The Normans and the Norman Conquest*. 2d ed. Wolfeboro, N.H.: Boydell. 1986. Pp. ix, 259. \$32.50.

In preparing the second edition of this book, which first appeared in 1969, R. Allen Brown has not changed the overall structure or main argument, but he has brought the book up to date by citing scores of new works. The book is still divided into six sections, dealing sequentially with eleventh-century Europe; Normandy before 1066; England before 1066; relations between England, Normandy, and Scandinavia; the Norman conquest; and Norman England. Brown has recast the section on early Norman history, which

now follows the arguments of John Le Patourel and Eleanor Searle (see p. 15, n. 1), and has slightly modified other passages in the light of other recent publications, including his own.

In making his revisions, the author has seen no need to withdraw his original claim, now reprinted, that "upon English historians in the twentieth century the influence of the Anglo-Saxons has increased, is increasing, and ought to be diminished" (p. 5). Still fearing that the Anglo-Saxon tide may engulf all but the hardest followers of the Conqueror and warning us once again that, "if it were necessary to take sides, I should be with duke William at Hastings" (p. 5), Brown now insists that "a firm statement of specifically Norman achievement, of *some* Old English weaknesses, and of profound change brought about by the Norman Conquest, is as necessary now as it was in 1969" (p. vii). Now, as then, Brown argues that by importing feudalism into England, where it had not previously existed, the rulers of Normandy, where feudalism existed well before 1050, transformed England dramatically and created a new world. Just as crucial to the attainment of Brown's polemical objectives are his efforts to inspire admiration for the Normans. He continually colors his prose with the poetic formulas favored by other modern romancers of medieval state building: "the foundations of modern government" (p. 9), "a new, restless and enquiring spirit" (p. 9), "restless existence" (p. 12), "the remarkable spirit of expansionist adventurism" (p. 12), "remarkable abilities" (p. 12), "dynamic energies" (p. 14), "dynamic energy" (p. 25), "organising ability" (p. 22), "remarkable unity and coherence" (p. 26), "that sense of communal identity and well-being" (p. 28), "forceful energy" (p. 28), "true political cohesion" (p. 50), "revival and high endeavour" (p. 222), "political unity" (p. 225), "a new, virile and militant aristocracy" (p. 225), "this heritage of blood" (p. 227), and, of course, "achievement" (*passim*). Because this kind of romance has no place for skepticism about Norman feudalism or Norman achievement, it is fitting that David Bates's *Normandy before 1066* (1982), which Brown views as an "attempt to denigrate the Normans" (p. 5, n. 15), "is but little cited" as Brown candidly explains, "because I disagree with so much of it" (p. 19, n. 18). Even readers who agree more with Bates than with Brown (without being followers of Harold or E. A. Freeman) will be struck by Brown's own achievement in putting his case for the Normans with so much learning and passion.

STEPHEN D. WHITE
Wesleyan University

W. L. WARREN. *The Governance of Norman and Angevin England, 1086–1272*. (Governance of England,

number 2.) Stanford: Stanford University Press. 1987. Pp. xv, 237. \$32.50.

W. L. Warren is primarily known for his excellent biography of Henry II (1973). He now has gone from triumph to triumph with a different type of book, broader in scope (from 1086 to 1272) yet written much more concisely. Warren proves himself a master of synthesis with his unobtrusive use of the secondary literature on the period punctuated by judicious quotations from the sources that provide apt illustration and confirmation of his points. Compared to many books on administrative or constitutional history, his fluent style also makes this book a pleasure to read.

Eschewing abstract principles, Warren takes a functional approach in explaining insofar as possible how the government really worked. His book also attempts to incorporate some of the insights of social history into an understanding of government: "Nowadays we tend to think that the kind of society any state has is determined by the type of government it has. In the Middle Ages the type of government it was possible to have was largely determined by the kind of society over which it was trying to rule" (p. 1).

This straightforward approach results in the clearest explanation I have seen of what effect the coming of the Normans had on English government. His description of how Anglo-Saxon lordship was modified by importation of the Norman institution of the fief is a tour de force neatly answering the traditional questions about feudalism without using the by now ambiguous terms "feudalism" or "feudal system." On the government of Henry II at the center of this period the reader could expect an authoritative account and would not be disappointed. In fact, the clarity of these chapters could even provide an excellent introduction to the more lengthy treatment in Warren's own biography of Henry. Pages dealing with the reign of King John serve as a reminder of his earlier biography of John published in 1961, a work he does not himself cite. In these pages he makes good use of both earlier and recent studies to provide a balanced and judicious account of John's government and of Magna Carta as a repository of the grievances against Angevin government.

With the reign of Henry III Warren confronts many of the issues discussed in traditional constitutional histories. His explanations are nuanced by placing both the king's and the barons' positions in the context of the way government operated in the thirteenth century. The section entitled "Crown and Community" provides a clear analysis of the dispute between the two factions and a revisionist conclusion on why neither party could claim to

speak for the "community of the realm" as that concept had developed over a century of Angevin government. He concludes that the problems of harmonizing these interests were not to be solved until the later development of Parliament when the Commons came to represent the communities of the realm.

This is a remarkable book—inclusive in embracing various approaches to understanding history, suggestive in analysis and new insights toward the solution of old problems, and instructive with its clear explanations based solidly on the sources while refusing to conceal difficulties when the sources are inadequate. Warren has set as high a standard for the remaining volume of "The Governance of England" series as he did with his biography of Henry II for the "English Monarchs" series.

CHARLES R. YOUNG
Duke University

SALLY N. VAUGHN. *Anselm of Bec and Robert of Meulan: The Innocence of the Dove and the Wisdom of the Serpent*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xxi, 392. \$45.00.

This work is a dual biography. Its aim is to examine the interaction between two great men of the Anglo-Norman world: Anselm, prior and abbot of Bec, later archbishop of Canterbury, and Robert of Meulan, knight and courtier in the service of King William I and Duke Robert Curthose, later chief adviser to King William Rufus and Henry I. Sally N. Vaughn, author of *The Abbey of Bec and the Anglo-Norman State* (1981), here focuses on Anselm as politician and statesman and develops a longstanding interest in the "remarkable achievements" (p. xiv) of Robert of Meulan. Her fresh look at the evidence, including Archbishop Anselm's extant correspondence of some five hundred letters, revises the view that Anselm was a politically inept theologian with little interest in worldly affairs. Vaughn also assembles and meticulously assesses the contemporary evidence about Robert of Meulan (though admittedly not as voluminous as that concerning Anselm) and argues for his equally prominent place in Anglo-Norman politics.

In the course of eight chapters, Vaughn develops the story of these two men, "whose lives were tightly intertwined" (p. 10). Following a prologue that introduces us to St. Anselm, Robert of Meulan, and the historical sources, chapters cover the careers of the two men from their first encounter in Normandy ca. 1088 over the rights of the Abbey of Bec through their later roles as adversaries in the church-state conflicts in England from 1093 to

1107. Vaughn moves skillfully through chapters that alternate between Anselm and Robert of Meulan, first considering their early careers and then moving to their dramatic confrontations in the unfolding of the investiture controversy. Consider the portrait that emerges: both were wise and devoted to law and justice, eloquent and persuasive; both were worthy diplomats, clever and prudent. "And both were seen to possess the outstanding virtue applicable to the role each played—Anselm's holiness and love of God, Robert of Meulan's loyalty and fidelity to his king" (p. 18).

Despite these parallels and similarities, there were sharp differences between the two that made them adversaries. Anselm was determined to carry out his duties as archbishop and held firm to the concept that king and archbishop should cooperate as "corulers of the Christian realm" (p. 150), each in his own sphere. Although Anselm is sometimes viewed as a Gregorian in his support of the papal reform movement, Vaughn emphasizes that a closer look will show that both he and Lanfranc shared similar ideas about Canterbury's relationship to Rome: "respectful but strictly limited subordination—obedience from a distance" (p. 153). Anselm's view of the right order of the world focused on God and church. It called for cooperation with the king, but he found in William Rufus an admittedly difficult personality. Still, the issues that separated Anselm and Rufus were not trivial. Insistence on receiving the papal pallium, holding councils, and protecting the lands of his see were all part of the Canterbury tradition. Anselm's vision was one of corule, but Rufus saw Anselm as an obstacle to his "absolute and unquestioned rule on England . . . Normandy and perhaps beyond" (p. 177).

In contrast to Anselm, Robert of Meulan emphasized "divinely sanctioned kingship responsible for ruling over the church" (p. 18). In the end, however, theirs were different paths to the same goals of peace, reform, and effective governance. Vaughn's portrait of her two subjects as two "great statesmen and pioneering diplomats" (p. 364) is a valuable contribution to Anglo-Norman studies and to the history of the English investiture controversy. The book contains maps and miscellaneous figures, includes two appendixes—William I's confirmation charter to Bec and genealogies of the Beaumont and Clare families—and concludes with an index.

PHYLLIS B. ROBERTS
College of Staten Island and Graduate Center
City University of New York

FRANK BARLOW. *Thomas Becket*. London: Weidenfeld and Nicolson. 1986. Pp. xii, 334. £14.95.

This is one of the half-dozen best books on medieval history published in the past decade. Although it leaves mostly unexplained the motivations of Thomas of London (as his contemporaries called him), this biography is a formidable achievement of historical scholarship and belongs in every American college library.

Frank Barlow's strength as a medievalist has always lain in his command of baronial lineages and ecclesiastical gossip, and these skills are put to good use here. His weakness as a medievalist—naiveté and uncertainty about literary tropes and spiritual formularies—also creeps in: did Thomas as archbishop of Canterbury really rise daily at two o'clock to wash the feet of thirteen paupers, and did he really have a dream while still in exile in France, four years before his death, that he would be killed by four knights? Nevertheless, Barlow has a sweeping command of a difficult and tempestuous life and exhibits shrewd insight into a very problematic personality. This book will endure.

The greatest value of this book is the gossipy detail that deromanticizes the twelfth-century church. Thomas was a dropout or flunk-out at the University of Paris; in spite of strenuous efforts at adult education while chancellor and archbishop, he never mastered the ornate ecclesiastical Latin of his day, and his official letters as archbishop were all written for him by his learned clerks after he gave them essential instructions (in French). Becket's rival, Archbishop Roger of York, while an archdeacon, kept a young boy for homosexual purposes, and, when the scandal broke, Roger brought trumped-up charges against the boy in an ecclesiastical kangaroo court and had the boy blinded and hanged. When the French county of Blois suddenly was inherited by an abbess in England, Henry II got some kind of ecclesiastical approval for the abbess to be forthwith removed from her convent and married off to a French nobleman. When Thomas was chancellor, although he was in ecclesiastical orders, he put on armor and led a feudal campaign in France. And, when Thomas was chancellor, his physician recommended sexual intercourse as a remedy for the strains of overwork. Barlow is unsure about Thomas's actual chastity on this and some other occasions; he may have had a liaison with one of Henry II's discarded mistresses. In 1166 the king became so frustrated with the querulous Thomas that he had a temporary royal breakdown, taking off all his clothes in public and chewing straw. Pope Alexander III was constrained from giving more than marginal support to Thomas because without Henry II's support he would not have secured the Throne of Peter against an imperial antipope, and if Alexander had supported

Thomas in the extreme manner the archbishop wanted Henry would have simply switched to the opposition pope. John of Salisbury, the most renowned humanist of the twelfth century and one of Thomas's clerks and later a principal biographer of the martyr, negotiated secretly with the king behind Thomas's back for several years, and, if the king had only been a little more generous, John might very well have betrayed the archbishop. When the four knights broke into the cathedral to kill Thomas, John of Salisbury ran away. When one of the knights ran up to assault Thomas, the archbishop called him a "pimp." The punishment imposed on the four assassins by the pope was fourteen years on Crusade, but at least one of them appears to have died a wealthy landowner in England. Because of his six-years exile in France, Thomas acquired so many habits of the French that, when he was fatally assaulted, he commended his soul not only to God and the Blessed Mary but also to St. Denis.

Barlow falls short of explaining convincingly what caused Thomas's erratic and self-destructive behavior. Barlow seems to prefer a social explanation. Thomas, a merchant's son of modest education, lived in a very high aristocratic world of unlimited greed, ambition, and lust; therefore, "he had all the failings of a typical parvenu" (p. 89). From the information Barlow provides, there are two other possible explanations. First, there appears to have been a deep psychosexual tension between Thomas and Henry II (fourteen years his junior). It is significant that the prime obstacle to a peaceful resolution of their quarrel in 1169, after two years of negotiation, was Henry's refusal to give Thomas a kiss of peace (on the mouth). Second, Thomas exhibits all the qualities now diagnosed as severe manic-depression or pathological moodswing. If Thomas could have had a dose of lithium each morning, there would have been no Canterbury martyr.

Barlow brings out forcefully Thomas's failures as archbishop and the hatred on almost every side, both ecclesiastical and lay, that he generated. "His rule as archbishop can be viewed as disastrous for all concerned" (p. 271). Thomas's reputation altered radically when the blood of the Canterbury martyr turned out to have almost incomparable curative qualities for every known disease. Soon, the selling of vials of water with an alleged tincture of his blood became a prime industry. Thomas himself never drank water; it bothered his stomach; he preferred the very best French vintage wine.

NORMAN F. CANTOR
New York University

P. R. CROSS and S. D. LLOYD, editors. *Thirteenth-Century England, 1*. (Proceedings of the Newcastle-upon-Tyne Conference, 1985.) Wolfenboro, N.H.: Boydell. 1986. Pp. 195. \$40.00.

This volume contains the proceedings of a conference on thirteenth-century England held at Newcastle upon Tyne in 1985. The "1" in the title poses something of a mystery. It suggests a series, yet the same press in 1985 published a volume of sixteen papers on England in the thirteenth century presented at a symposium held at Harlaxton College (the British campus of the University of Evansville, Indiana) in 1984. Two scholars have papers in both volumes.

At the Newcastle conference there were four main papers: J. R. Maddicott's "Edward I and the Lessons of Baronial Reform: Local Government, 1258-80," Michael Prestwich's "Royal Patronage under Edward I," D. A. Carpenter's "The Gold Treasure of King Henry III," and Edward Miller's "Rulers of Thirteenth Century Towns: The Cases of York and Newcastle upon Tyne." These are all substantial contributions—particularly the first—and, taken together, fill 84 of the book's 195 pages. In the remaining pages we are treated to such diverse topics, ranging from the serious to the merely antiquarian or downright picayune (and, apparently, not all the papers presented at the conference are included here), as Edward I and the judges, 1289-93 (Paul A. Brand), fiscal uses of royal wardship under Edward I (S. L. Waugh), the Lord Edward and the Provisions of Oxford (Huw Ridgeway), the end of the Norman earldom of Chester (Richard Eales), the number and distribution of knights (J. Quick), provision for the families of the Montfortians after Evesham (Clive G. Knowles), the place of Castile in the commerce of northern England (Henry Summerson), the making of Dublin into a colonial capital city (J. A. Watt), Ireland and the Barons' Wars (Robin Frame), some English evidence of attitudes to crusading (Christopher Tyerman), the social context of vernacular writing (John Frankis), and the South English Legendary (Annie Samson). Note that nothing is said about the reign of King John.

There is no theme in this volume, and the focus "provided by the studies of the relationship between the Crown and its subjects, the relationship between central and local government, and aspects of contemporary society," to quote editors P. R. Cross and S. D. Lloyd, is no substitute. Nor is there any index.

Perhaps these criticisms should really be addressed to those who planned the Newcastle conference. Compare this volume with another published by the same press in 1987, which is a model of its kind and which is an enduring contribution

to historical scholarship: *Domesday Studies*, edited by J. C. Holt.

G. P. CUTTING
Emory University

G. P. CUTTINO. *English Medieval Diplomacy*. Bloomington: Indiana University Press. 1985. Pp. vii, 162. \$25.00.

At the beginning of this book, G. P. Cuttino very wisely points out that the usual modern meaning of the word "diplomacy" dates from as recently as 1796. Therefore, the period on which he writes saw conventions and practices very different from those of modern times. His first chapter (by far the best in the book) discusses changing diplomatic formulas from three periods in an extremely interesting way: an agreement of 1100 between Henry I of England and Robert of Flanders was not so much a treaty in the modern sense as a personal agreement of a feudal nature; by the time of the Treaty of Paris of 1259, as part of a swift transition to government by written record, agreements, though still royal and personal documents, were couched in certain diplomatic-cum-legal formulas that involved the affairs of England as a whole—sealed with green wax, a color that denoted permanence; and by 1300 personal-cum-feudal relations were developing to something, in Cuttino's opinion, definitely tinged with nationalism. It is impossible in a brief précis to do justice to the subtlety of the verbal arguments involved. However, the topic of nationalism in the later Middle Ages is one that has aroused passionate disagreement among several generations of historians, and, in my opinion, Cuttino very much exaggerates its positive aspects. I prefer to see the development as dynasticism drumming up propaganda against aliens, a much more negative concept. Others would no doubt disagree.

After this early section the book, in spite of some fascinating details, is somewhat disappointing. Cuttino has a very considerable command of both original documents and secondary works. Controversial interpretations, particularly those involving points of feudal law, are clearly discussed in detail, but the various chapters tend to become bogged down in distinctly pedestrian diplomatic narrative. The book is mainly an account of Anglo-French relations, and its main defect, and a very serious defect, is a complete absence of any discussion of the economic resources of both sides that made possible their confrontations. In the epilogue Cuttino remarks that on mercantile affairs there was much shrewd bargaining but that "English policy was by and large successful enough but, like most things economic, exceedingly dull, except to those

who made the profits. And, by and large, the English mercantile class was happy" (p. 115). The second of these statements is demonstrably incorrect, and the first all too reminiscent of that blinkered class of academics who snobbishly despise the commercial and industrial classes who provide the tax base that makes possible their own intellectual activities and certainly made possible the diplomacy and warfare of England and France in the Middle Ages. Wars and diplomacy did not exist in economic vacuums, and one cannot validly discuss diplomacy as if they did.

J. R. LANDER
London, United Kingdom

KATHARINE SIMMS. *From Kings to Warlords: The Changing Political Structure of Gaelic Ireland in the Later Middle Ages*. (Studies in Celtic History, number 7.) Wolfeboro, N.H.: Boydell. 1987. Pp. ix, 191. \$35.00.

The past dozen years have seen a number of highly controversial attempts to exploit a relatively unfamiliar source, Gaelic bardic poetry, for the light that it may throw on the changing climate of political ideas in early modern Ireland. Katharine Simms's work is perhaps less ambitious but more systematic and solidly based. Building on her own specialist articles in Irish periodicals, she applies similar techniques to offer a wider audience a comprehensive account, within the severe limitations of the evidence, of the institution that provided the *raison d'être* of these poems, Gaelic lordship in later medieval Ireland. In nine chapters she discusses the sources and political background; the choosing, inauguration, and deposition of kings; the character of kingship, vassalage, and submission; and the king's administration, counselors, army, and revenues. Her thesis is that, under the impact of war and foreign invasion in the High and later Middle Ages, the traditional theories and practice of kingship in Gaelic Ireland, upholding an essentially limited, aristocratic concept of kingship, gave place to more arbitrary and absolute forms. There developed instead a species of warlord, dependent on a professional army and paid officials, and with much increased claims on his subjects. The warlord's position and authority depended far more transparently on armed might than on descent and right rule.

By and large the author is able to extract enough evidence from some extremely recalcitrant source material to support her conclusions. She also offers many other important insights into the nature of Gaelic politics and society. Yet the reader frequently has to pry out the major findings from a mass of detail. Presumably, this re-

flects the book's origins in a doctoral dissertation. Parts of the later chapters refer back constantly to earlier sections and add little to our knowledge of developments. Moreover, few historians will share the author's obvious enthusiasm for the obscure technicalities of bardic verse, and some remarks, such as those about nouns with "o-stem" and "u-stem" inflections and differing genitive singulars should really have been consigned to a grammar book (p. 84). Thus, the book is hard to use, the more so because personal and place names appear in Gaelic, although there is an extensive and very necessary glossarial index of technical terms. More worrying is the failure to address adequately the problem of periodization and the motivation behind particular developments. The book ranges over almost a millennium, but apparently half the poems date from the period after 1565 when the Gaelic system was under increasing military strain. No doubt the author was careful in her use of sources, but even the chronology of English expansion and decline in later medieval Ireland is currently quite controversial. A wider perspective might have helped settle doubts; for example, Gaelic Scotland figures only incidentally, although Scottish chiefs surely faced similar problems to those of their Irish counterparts; and the increasing emphasis on the surname (chap. 3) recalls the contemporary emergence of surnames on the Anglo-Scottish borders. Nevertheless, this is, overall, a very substantial and much needed addition to the pioneering work of Kenneth W. Nicholls and Gearóid Mac Niocaill on later medieval Gaelic Ireland.

STEVEN G. ELLIS
University College
Galway, Republic of Ireland

ROGER STALLEY. *The Cistercian Monasteries of Ireland: An Account of the History, Art, and Architecture of the White Monks in Ireland from 1142 to 1540*. New Haven: Yale University Press. 1987. Pp. vii, 295. \$40.00.

Roger Stalley here pioneers a subject, the buildings and art of the Irish Cistercians from the arrival of monks from Clairvaux to found Mellifont in 1142 up to the dissolution of the order. Unlike Peter Fergusson's recent *Architecture of Solitude* (1984), Stalley's book does not concentrate on churches but includes all the main claustral buildings, as well as tiles, metalwork, seals, painting, and illumination. He provides much information, most of it previously unknown to more than a small circle, generously illustrated with photographs, plans, and line-drawings. Full notes, not too easy to consult at the back of a large book

(about twenty-three by twenty-nine centimeters), bibliography, gazetteer of main sites, glossary, and index complete a handsome book—ornament to its publisher's skill.

This book is important, introducing a theme that probably has more significance for the general history of Ireland than for other countries, since the Cistercians introduced new ideas about the scale and cohesion of monastic buildings and established themselves in both the Celtic and Anglo-Norman areas. They played a central role in the diffusion of plans and styles to other patrons and builders, a process that Stalley reveals clearly. He highlights, for example, the influence of the Fontenay church plan over a long period and shows similarities with techniques used in south Wales. He manages to make the mysteries of vaulting comprehensible and tackles with élan the much discussed issue of whether the Cistercians planned their churches with a set of proportions in mind. I would have been more convinced by his conclusion that they did if he had consistently stated where he was measuring from—the outer side, inner side, or center of a wall—and at what height—present or assumed medieval surface. Yet on the whole he seems a cautious guide who openly reveals the evidence and has the gift of a good phrase.

His touch is less sure with wider scenes, and, despite his subtitle, he does not provide a history of the order but a splendid study of art and architecture. Even this has its surprises. Why, for example, does he not mention the widely held view that Cistercians swung the refectory to a north-south axis to leave space for a kitchen to serve both monks and lay brothers in their separate quarters? Sometimes, too, he describes changes in a rather unsatisfactory way, as when he writes that the nave at Boyle is "stylistically in advance of the equivalent parts of Baltinglass and Jerpoint" (p. 89). This seems to rest on the arches being "more elegant" and the design of the pillar bases marking "a development." Are such words convincing for a society that showed great attachment to old styles? But such doubts must not weaken the welcome and praise that this book deserves.

CHRISTOPHER HOLDSWORTH
University of Exeter

LYN RODLEY. *Cave Monasteries of Byzantine Cappadocia*. New York: Cambridge University Press. 1985. Pp. xviii, 266. \$79.50.

The cave monasteries of Cappadocia in central Asia Minor provide a remarkable conjunction of geological and cultural phenomena. The land-

scape of the area was formed by the erosion of soft volcanic deposits, naturally resulting in fantastic cones and outcrops, which were then hollowed out and used by local inhabitants over the centuries. Despite the impressive nature of the remains, however, there are no literary sources that mention them, and analysis can be made only on the basis of observation. Although considerable work has been done on the paintings of these monuments (for example, N. Thierry and M. Thierry's *Nouvelles églises rupestres de Cappadoce* [1963] and A. W. Epstein's, "Rock-cut Chapels in Göreme Valley, Cappadocia: The Yılanlı Group and the Column Churches," *Cahiers archéologiques* 24 [1975]: 115–35), Lyn Rodley's book is the first of general interest for the historian. In terms of methodology, it provides an example of how mute architectural evidence can be used to help elucidate the religious and social life of a local society that has otherwise vanished completely. This book, then, stands on the threshold of history and archaeology, with enough of both to keep most practitioners happy; to its credit, the book is free of jargon and can be read by specialists in fields beyond the narrow scope of Byzantine studies. The analysis focuses exclusively on the monasteries (as opposed to simple churches or other buildings), and the core of the book is a set of detailed descriptions of each of the extant complexes, grouped according to a typology based on form (courtyard monasteries, refectory monasteries, hermitages). This work in itself is a useful service to scholarship since many of the structures have not been properly documented. Basing her analysis on this evidence, the author then goes on to attempt what is essentially a history of the monasteries and the social, economic, and political conditions that gave rise to them.

The book begins with an examination of the alleged "villages" and "underground cities" frequently noted in the area, which have been associated with instability and flight from marauders. Rodley reasonably points out that most of these underground chambers cannot have been occupied in any period, and she identifies them as simple storage areas. In the absence of documents or stratified excavation, decoration, especially painting and architectural detail, provides the main evidence for chronology. This is, of course, necessarily imprecise and subject to reconsideration, but enough work has been done on securely dated monuments to allow the method to be used with some certainty here. On this basis Rodley argues that the hermitages were the earliest type of complex in the region and the one with the longest life, dating from the late ninth or early tenth century. The earliest monastery churches can be assigned to the eleventh century, while the

similarity of layout and detail suggests that all of the monasteries should be assigned to a fairly short chronological span: thus, the fully developed monastic complexes were begun just a few years before the Seljuks began their conquest of Anatolia, explaining the short life of the communities and their failure to find a mention in the surviving sources.

The construction of these rock-cut complexes must have been carried out by specialists, and the work supported by local patrons (a few of whom are commemorated by portraits in the churches). Although there are significant differences in the means of construction, the rock-cut churches find significant parallels in both Mesopotamia and Constantinople. The author argues that the "courtyard" monasteries were established by their patrons essentially as funerary monuments and that the less elegant "refectory" monasteries were constructed to provide accommodations and assistance for visitors to the local holy sites. The monasteries were thus built in a brief moment, when economic and military conditions were just right and when traffic through this desolate region could provide the necessary support. The remains are not elegant, and today they are famous primarily as curiosities for tourists. Nevertheless, as the book concludes, "they endure as material evidence of a generally unchronicled Byzantine class: monks who lacked hagiographers; soldiers too low in rank to be given credit for victories; minor officials who did nothing good, bad, or important enough to bring them to the attention of historians." This book serves the important task of preserving this evidence and providing an example of how historians may begin to analyze it.

TIMOTHY E. GREGORY
Ohio State University

TIMOTHY S. MILLER. *The Birth of the Hospital in the Byzantine Empire*. (Henry E. Sigerist Supplements to the Bulletin of the History of Medicine, new series, number 10.) Baltimore: Johns Hopkins University Press. 1985. Pp. xv, 288. \$19.95.

Timothy S. Miller's primary thesis is that the Byzantine empire developed institutions for the care of the sick that resemble modern hospitals much more closely than do those of medieval Western Europe or of the ancient world (pp. 3, 210-11). The criteria used to define a modern hospital, and therefore to posit the similarity between it and the Byzantine *xenon*, are the fact that the hospital aims specifically at the care of the sick, rather than being a poorhouse or an old-people's home, that professional care is given to the patients, and that patients of all classes, rich and

poor, are accepted. All of these elements he finds in the Byzantine hospital, and he particularly stresses the last point, partly because he believes that an institution that cares for the wealthy as well as the poor is apt to provide quality medical care (chap. 2).

The best-known Byzantine hospital is that attached to the monastery of Pantocrator, a twelfth-century imperial foundation, and quite properly the author begins his study with an examination of this institution. He then looks for similar traits in the description of other philanthropic institutions, to establish whether these were organized on lines similar to those of Pantocrator; this methodology is not without dangers, as can be seen from the discussion of the charitable foundation of St. Basil of Caesarea (p. 87).

After discussing the hospital of Pantocrator, the author examines the care of the sick in the ancient world (chap. 3), the role of Eastern Christianity in the development of the hospital (chap. 4), and the changes that occurred in the fourth century (chap. 5). There is a somewhat confusing description of conflict between hospitals and healing saints (p. 65) and an interesting, though not entirely persuasive, discussion of the influence of Arianism on the development of Christian charitable foundations. The remaining chapters examine the role of the church, monasteries, and imperial and private philanthropy in the development of hospitals over time. Chapter 9, on hospitals and medical literature, tries to establish a connection between medical practice and the development of medical knowledge.

This is a useful survey, which brings together a significant amount of information. It suffers from considerable repetition, to some extent because the available information is relatively limited, and therefore the same texts are used in different parts of the book to make the same argument. There are, occasionally, some curious mistakes. Thus, the nonexistent Greek verb "tropho" is mentioned twice (p. 24) in a discussion of terminology, and the well-known jurist Eustathios Romaos appears as "Romanos." Finally, the subsidiary thesis of the book, that is, that the hospital is the most fascinating institution of the Byzantine empire (p. 11) and that it functions as a prism through which one gains powerful new insights on Byzantine life and thought, is, in my estimation, a somewhat exaggerated claim.

ANGELIKI E. LAIOU
Harvard University

MIRI RUBIN. *Charity and Community in Medieval Cambridge*. (Cambridge Studies in Medieval Life and Thought, fourth series, number 4.) New

York: Cambridge University Press. 1987. Pp. xiv, 365. \$49.50.

Miri Rubin's book is a fine addition to a growing and very important collection of studies on disadvantaged groups and their treatment in medieval society.

The core of her study is the Hospital of St. John the Evangelist in Cambridge, whose rich archives permit an understanding of the intention and functioning of bequests over a period of some three hundred years, from its foundation in the early thirteenth century until the hospital was dissolved to make way for an academic college. Rubin uses these materials especially to reveal the changing spiritual needs of donors and the results for the poor. During the twelfth and early thirteenth centuries these needs were expressed by "indiscriminate charitable giving based in endowed institutions" (p. 291), but they deteriorated dramatically after the Black Death into a much narrower concern for liturgical rather than social benefits. Thus, the Hospital of St. John really only received major endowments in the first few decades of its existence. By the fifteenth century, despite astute management of bequests, the hospital was acting almost exclusively as a chantry and a boardinghouse for scholars and commoners; it had thus ceased entirely to care for the poor and sick, and its dissolution scarcely affected the quality of life of the Cambridge community.

Broadening her vision to include as much of the charitable activity in medieval Cambridge as can be perceived through documentary sources, Rubin finds a similar pattern: as time went on, many almsgivers increasingly looked for different spiritual gains, in the form of specific prayers by the recipient poor or, more frequently, by clerics. To explain the phenomenon, Rubin expands her vision still further, to include all of Europe, both in terms of socioeconomic history and in terms of religious teachings. She observes that the Black Death created a turning point in attitudes toward charity. By reducing the number of potential workers and at the same time the number of tenants for rural and urban holdings, the plague cut into the income of the wealthy who were the givers of charity and also changed their image of the lower classes from that of general poverty to that of general prosperity. People who were not working were no longer seen as valid participants in the divine order of life on earth but rather as hardened vagrants, and doubt was therefore cast on the ability of the poor to fulfill their spiritual function of justifying the wealth of the rich by being worthy recipients of charity. As a result, the much more limited concept of the "deserving poor," whose merits could be ascertained through

the parish, guild, or college framework, replaced the less discriminating largesse of the earlier period. Increasingly, in fact, care of the poor was seen as a municipal obligation, while spiritual benefits from almsgiving were connected with funerary and memorial activities.

The only difficulty with this line of reasoning is that there is a good deal of evidence to show that charitable foundations, in Paris and Poitiers for example, continued to function according to their original statutes as hospitals or orphanages, with spiritual as well as social functions, throughout the fourteenth and fifteenth centuries. Thus, apparently not all areas of Europe experienced the same economic dislocations or reacted to them spiritually in the same ways. As is the case with other aspects of medieval life, global explanations will have to await many more local studies.

But this is a minor observation to make regarding an important work that not only provides us with an excellent summary of the relevant literature but also greatly advances our knowledge through a meticulous analysis of almsgiving on a local level. The bibliography is comprehensive, the index thorough and intelligent, the writing clear and engaging. There is also a map of fifteenth-century Cambridge: crucial but often omitted in historical studies.

ELLEN WEDEMAYER MOORE
John Abbott College

JAMES M. POWELL. *Anatomy of a Crusade, 1213–1221*. (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1986. Pp. xix, 287. \$34.95.

This volume presents an interesting and often novel and innovative approach to the interpretation of the background and events of the Fifth Crusade. The opinions expressed frequently derive from the reinterpretation of controversial evidence and will in turn lead to further controversy. James M. Powell places the Crusade in the context of the West European peace movement led by the papacy. The major actors are Popes Innocent III and Honorius III, Emperor Frederick II, King John of Jerusalem, and the papal legates, especially Robert Courçon and Pelagius. The text is divided into two parts, concerning the preparations for the Crusade and the Crusade itself. One of the four appendixes consists of a valuable list of some eight hundred fifty names of individuals who took the cross, including where possible their dates of arrival in, and return from, the East. These names have been analyzed quantitatively in chapter 9 ("The Conquest of Damietta") to show the relative numbers and status of crusaders who arrived in the East in the

spring and fall passages between 1217 and 1221 and their overall rates of mortality and survival. Additional information about a number of these and other individuals allows the author to incorporate them as examples in his case-study approach to situations leading up to the Crusade.

Working in the shadow of the failures of the Third and the Fourth Crusades, Innocent III determined that the success of a new expedition depended on careful planning. It was his objective to substitute Crusade service for internal conflict. The program led to the foundation of a Crusade theology that promoted the movement as "a fitting instrument for the moral transformation of the individual Christian and of Christian society as a whole" (p. 63). In 1213 he began preparations for the Fourth Lateran Council, held in November 1215, which set the stage for church reform and the establishment of peace through the promotion of the Crusade. The movement was to be financed through the taxation of the clergy and through the redemption of crusader vows taken by those to whom Innocent had extended the plenary indulgence but who were obviously unable to journey to the East. A crucial problem unresolved at the time of Innocent's death was that of leadership. It was Honorius III who, continuing Innocent's desire for royal command, designated Frederick II to lead the Crusade army. The two cooperated in this intent until 1221, by which time, after four years in the field, the Crusade army that had succeeded in taking Damietta was obliged to relinquish its gains and return to Europe. The author insists that the blame for this new failure is to be laid neither on inadequacies in papal planning or procedure nor on command disagreements in Egypt but rather on the particularism and violence of European society, which prevented adequate reinforcements, funds, and the emperor himself from reaching the army when it most needed them.

MICHAEL GERVERS
University of Toronto

JOSEPH CANNING. *The Political Thought of Baldus de Ubaldis*. (Cambridge Studies in Medieval Life and Thought, fourth series, number 6.) New York: Cambridge University Press. 1987. Pp. xi, 300. \$49.50.

Of all medieval jurists Baldus (1357?-1400) was the most philosophical, the most political, and (except for his teacher Bartolus) the most influential. Yet this is the first critical and comprehensive study of Baldus's extensive work (commentaries on all the major sources of law and twenty-five hundred *consilia* totaling an estimated 7 million

words). Joseph Canning's excellent book, based on a dissertation written under the supervision of Walter Ullmann, surveys Baldus's ideas on a number of topics significant for modern political thought: sovereignty, civic government, various social groupings, citizenship, and, especially, the field of "political science," its terminology, its questions, and its conceptualizations.

Baldus was one of the earliest celebrators of the "political man" (*homo politicus*). The belief that human beings were not merely natural and economic (possessing a body and a soul and located in a family) but also communal was of course common in Aristotelian and Ciceronian political philosophy, but it was jurists such as Baldus, Bartolus, and Guilelmus de Cuneo who, extending the common notion of "legal science" (*legitima scientia*), worked this out in concrete social, legal, and institutional contexts.

Here, although he does not argue the point, Canning makes an important contribution to contemporary discussions of "civic humanism," which have been associated with Renaissance rhetoric and politics and (perhaps questionably) projected as far forward as the American and French revolutions, but which have all too seldom attended the legal tradition, so seminal and so authoritative in matters "civic" and political. Baldus was no humanist, but he had a sense of historical change ("as acute as he was lacking in learning," Vico remarked), and he was ingenious in adapting ancient legal devices to the empire, church, and "city-communities" of the fourteenth century.

Unlike Ullmann, J. G. A. Pocock, Quentin Skinner, and other contemporary historians of political thought, Canning prefers a short-term—an external rather than an internal—view of his subject. Canning studies Baldus as "the original jurist directly" rather than as "the jurist of historical tradition," emphasizing his contradictions and ambiguities and his surrender to medieval political conventions. Baldus did not (like the humanists) lament the obsolescence of classical institutions but rather accommodated them to "trecento political reality"; and, although he recognized the "higher norms" of natural and divine law, he also accepted prescription (for example, regarding the Donation of Constantine), custom (the creations of the *populus* of his own day), and de facto arrangements that also produced legitimacy. On the question of sovereignty, for example, Baldus's view was differential and "hierarchical" rather than "dualistic" (like that of Dante and Marsilius of Padua), and he took into account civic, feudal, and royal as well as imperial and ecclesiastical authority.

Historically, Canning's analysis is learned as well as acute; conceptually, it is more limited. One major problem he confronts only indirectly, for it

is only as a "jurist of historical tradition" that Baldus is really a political thinker. "He was not an apologist for any one form of government," Canning acknowledges, "but rather sought to provide an explanation for the forms of government which existed in his own day" (p. 15). Arguing *utriusque juris* (unlike Bartolus, Baldus was a master of canon as well as civil law) and *de facto* as well as *de jure*, Baldus distributed his *opiniones* over many disparate texts and *concilia* written for particular interests and contingencies; his work was largely an *oeuvre de circonstance*. His "original" achievement (as Vico among many others saw) was to give intelligibility and legitimacy to the "new" law of nations that legal science extracted from the sources of ancient civil law and accommodated to medieval and modern European conditions, but in fact he was only one of the initiators of a process that was to go on for centuries. The truth is that much of Baldus's significance for modern "political thought" is derived from the posthumous life of his work, and—although Canning has provided a valuable historical foundation—this remains to be explored.

DONALD R. KELLEY
University of Rochester

ANTHONY KENNY, editor. *Wyclif in His Times*. New York: Clarendon Press of Oxford University Press. 1986. Pp. vi, 174. \$38.00.

To celebrate the six-hundredth anniversary of John Wyclif's death in 1384, a program of lectures was arranged at Balliol College, Oxford, of which he was once, briefly, master. These lectures, reinforced by others relevant to the occasion, now appear in print to give an unusually full set of insights into the many-sided achievements of the man. None of the pieces provides more than the briefest nod at biographical information, however; for that one still needs to go to the standard older works or to the slim 1985 study in the Oxford "Past Masters" series by Anthony Kenny, himself just retiring as the master of Balliol and the editor of the present volume.

That the biographical dimension is touched on only scantily and unsystematically means that there is, for example, no concerted treatment of the context for the attack on Wyclif at the Earthquake Synod of 1382 nor of the extent of his connections with John of Gaunt. Historians' concerns are, however, superbly served by Anne Hudson's piece on Wycliffism in Oxford from 1381 to 1411, in which she shows that "the recent view that Lollardy was a spent force in Oxford by . . . 1384 needs substantial reconsideration" (p. 83), and by Maurice Keen on Wyclif's influence in the thirty

years or so after his death, which deploys evidence to be deduced from early manuscripts of Wycliffite writings in both England and Bohemia. Keen's article helps flesh out Gordon Leff's doctrinal comparison of the positions of Wyclif and Jan Hus and also complements nicely a second, and most accomplished, essay by Hudson on Wyclif and the English language.

Two of the pieces make difficult reading for nonphilosophers. Kenny's "The Realism of the *De Universalibus*" (of which his English translation has recently appeared) puzzles by failing to explain words such as "instantiation" and "supposit." Norman Kretzmann's essay, "Continua, Indivisibles, and Change in Wyclif's Logic of Scripture," suffers from a seriously misleading title—it has virtually nothing to do with (the logic of) Scripture—and appears to have strayed out of a journal of the most technical kind of philosophy.

The first and last essays set and close the scene fittingly. Maurice Keen leads off with an informative consideration of "Wyclif, the Bible, and Transubstantiation," and Kenny concludes with a catchingly, but like Kretzmann's misleadingly, titled piece, "The Accursed Memory: The Counter Reformation Reputation of John Wyclif." Two-thirds of the latter article actually deal, most interestingly, with the condemnation of Wyclif at the Council of Constance in 1415; less than half is concerned with events of the sixteenth century.

The usual verdict on volumes of this sort is that they are of varying quality. In this case each of the contributions seems to be of high quality; the variation is rather in the degree of usefulness to different audiences. For historians this book, despite being a mixed bag, is probably among the half-dozen most useful works on Wyclif, but we still need a big modern study of him.

RICHARD W. PFAFF
University of North Carolina

EVA DE VRIES—VAN DER VELDEN. *Théodore Métochite: Une réévaluation*. Amsterdam: J. C. Gieben. 1987. Pp. ix, 276. f. 85.

The publication of Eva de Vries—van der Velden's doctoral thesis is most welcome. Her innovative and refreshing approach adds new insights into the person and ideas of a distinguished Palaeologan figure, Theodore Metochites, who at the beginning of the fourteenth century emerged as a prominent Byzantine "man for all seasons." A modern literary historian, Karl Krumbacher, refers to him as one of the most prominent polyhistor of the Byzantine renaissance. To cite but some of Metochites' many accomplishments and attributes, he was well educated and gained fame

and recognition as a teacher *par excellence*, classicist, Platonist, poet, philologist, statesman, envoy to the Serbs, and minister to the Byzantine emperor, Andronicus II, who had complete confidence in him.

The study of de Vries, however, is uneven in its treatment. She begins with a comprehensive historiographical introduction, concentrating mainly on the scholarly writings and interpretations of four contemporary literary historians—H.-G. Beck, I. Ševčenko, H. Hunger, and M. Gigante. She follows with two main chapters: the first an extensive biographical essay and the second a discussion of the metaphysical and moral ideas of Metochites, which in turn are contrasted with the thought and writings of Plato and Aristotle. Next, two brief chapters examine the literary contributions and the politics of Metochites. The work also contains three appendixes, which, though brief, supplement the main chapters and focus on Theodore de Montferrat, the important post of *mesazōn* and the richness of the contributions of Metochites.

De Vries does not accept what she regards to be the very general and imprecise conclusions of Beck. She holds that his paraphrases of Metochites are inexact but strikingly imaginative. She is more kind to Ševčenko and regards his studies of Metochites, particularly his monograph that appeared in 1962, as an important contribution (pp. 6–11). Further, while Hunger identified Metochites as one of the humanists of the Palaeologan era (p. 11), de Vries believes that his definition of humanism lacks clarity in his three published articles. Lastly, she evaluates the scholarship of Gigante, whom she treats in more complimentary terms. Her substantial contrast of the scholarly contributions of these four literary historians is most significant and deserves the attention of anyone who studies the Byzantine renaissance.

The biographical essay in particular is rich in detail and begins with an extensive discussion of George Metochites, the father of Theodore. A question must be raised, however. Does de Vries intend to establish the notion that the social, political, and intellectual currents in Byzantine society and even George Metochites molded Theodore and guided his development into numerous fields of endeavor? This would negate the conclusion that Theodore himself developed his unique skills—an argument that I favor. On the other hand, de Vries does explore Theodore Metochites' philosophical observations with clarity and readability. Her study should renew and stimulate further discussion of this singular figure of the Byzantine renaissance.

WALTER K. HANAK
Shepherd College

ALOIS NIEDERSTÄTTER. *Kaiser Friedrich III. und Lindau: Untersuchungen zum Beziehungsgeflecht zwischen Reichsstadt und Herrscher in der zweiten Hälfte des 15. Jahrhunderts*. Sigmaringen: Jan Thorbecke. 1986. Pp. 156. DM 48.

The reputation of the late medieval German empire has suffered from the traditional concentration of German historiography on the High Middle Ages and the Reformation: compared with the glories of the Staufer or Charles V, who set their mark on all of European history, the late medieval kings seemed impotent, provincial, and uninteresting. The worst marks of all were awarded to Frederick III, the "weakest weakling" (Friedrich Baethgen) ever crowned king of the Romans, whose incompetence, passivity, and utter indifference to the fate of the empire pointed to his long reign (1440–93) as the nadir of medieval German history. However, largely because of the studies of Peter Moraw, this picture of the late medieval empire has been changing, as interest in its constitutional history has picked up.

One expression of this renewed interest has been a series of dissertations supervised by Heinrich Koller, whom the Austrian Academy of Sciences commissioned some years ago to calendar the acts of Frederick III for the *Regesta Imperii*. These Salzburg dissertations have focused on the relations between individual imperial cities and Frederick III and follow a more or less uniform outline, whose points reflect the influence of the questions Moraw has posed. Thus, Alois Niederstätter sifts the evidence (largely found in the archives of Lindau, Munich, and Neuburg an der Donau) for the payment of imperial taxes and other forms of aid (notably military) granted to the emperor; on the privileges Lindau received from Frederick; on his role as supreme judge in the empire (particularly in Lindau's conflicts with its territorial neighbors and rivals, the counts of Montfort and Duke Sigmund of Austria), on the imperial mandates addressed to the town (notably in regard to the foundation of the Swabian League as a counterweight to the Swiss confederation), and, finally, on the relations between individual citizens of Lindau and the emperor.

Although these topics, and indeed Lindau itself, may—from a North American perspective—seem impossibly obscure, they are in fact highly important. Neglect and deliberate policy had reduced imperial possessions since the mid-thirteenth century, until they consisted of little else than the imperial cities. Hence, the fate of the German monarchy in the late Middle Ages depended on the ability of kings to activate their financial resources in those cities and to bring their rights there into play, indeed, if possible, to expand

them. Lindau is a particularly good test case in this regard. As a Swabian imperial city, it kept a wary distance from the monarchy but lay, ominously, within the sphere of influence of the Habsburg family (if not of Frederick himself) and the Swiss confederation.

Thus, success in prosperous, independently minded Lindau was both a symptom of and a prerequisite for a more general revival of vigorous imperial government. Frederick III was able to bring his rights into play here and indeed to expand them modestly, as Niederstätter shows. In so doing, he has dealt with a number of complex issues, notably the conflicts between Lindau and the counts of Montfort, very well indeed. He has a particularly good grasp of the interplay of politics, the constitution, the legal system, and the "selfish" interests of the imperial cities. These are, moreover, issues that clearly far transcend the history of one small town on the shores of Lake Constance, and Niederstätter is careful to place Lindau's history in the context of regional, imperial, and European developments. Hence, the importance of his work is much greater than its title would seem to indicate.

Although this book is to be recommended to all students of late medieval monarchy, it would have been improved by the inclusion of a map and a more detailed discussion of Lindau's economy, which is merely said to have prospered.

STUART JENKS
Universität Erlangen-Nürnberg

ROBERT FOSSIER, editor. *The Cambridge Illustrated History of the Middle Ages*. Volume 3, 1250–1520. Translated by SARAH HANBURY TENISON. Reprint. New York: Cambridge University Press. 1987. Pp. xviii, 554. \$39.50.

This synthesis is arranged in three parts. The two dealing with Western Europe are divided chronologically at 1430 and are separated by a section on Europe's eastern and southern frontiers that is nearly as long as the others combined. The sections on Byzantium, Islam, and the Turkish and Mongol worlds have a discernible chronological organization and deal mainly with diplomacy, politics, and military matters. Briefer treatment is given to economic and social developments and to cultural history. The decision not to consider the West in isolation is commendable, but, in view of the detail lavished on Europe's southern frontiers, I found surprising the extremely cursory treatment given to Russia and Poland.

Jacques Verger provides a sober and balanced summary of cultural developments before 1430. The other five chapters in the two sections devoted

to Western Europe were written by Robert Fossier. Although I have immense respect for Fossier's previous work, these essays are disappointing. The organization is extremely fragmentary, and typographical and factual errors abound. (How can any reviewer recommend a book that, to cite only two of the many egregious blunders, places the origin of the English coroner in the reign of Edward IV and claims that Edward III of England arranged the assassination of James van Artevelde?) Although there is useful material in the social and economic analysis, particularly in the sections concerning the peasantry, it is dominated by notions of class struggle and Fossier's fondness for paper tigers. He dislikes "crisis of feudalism" as an analytical framework and prefers "disintegration of the manorial system." Since his analysis has nothing to do with feudalism, and there was never a manorial "system," I fail to see the point. Some theses are internally contradictory. Fossier claims that peasants' holdings were too small to produce a surplus for market and then says that their market sales were cutting into what had been the lords' monopoly. He is forced to admit that the urban rebellions had a strong political-party and family basis but then insists that they were really workers' rebellions.

Periods and entities are personified (for example, on page 521, "The Middle Ages, whose reasons for existing died one after another"), and Fossier seems to think that the question of when the Middle Ages ended is still of burning concern. I was not previously aware that "men could claim the right to work" in later medieval Europe (p. 9), and his discovery of "the liquidation of Christianity in the thirteenth century" will surprise many (p. 63). The blithe non sequiturs and mixed metaphors of the *Annalists*, which pass in some circles for profundity, appear even sillier in translation than in the original language. I was particularly entertained by the spectacle of "empty bellies piled up in inadequate holdings" (p. 50) and the thought of men "partly spared by death" (p. 89).

The illustrations, most of them in the sections on Western Europe, are helpful adjuncts to the text. Most have not been printed frequently, and a few were completely new to me. Yet the translation is often surprisingly unidiomatic. Most important, the book is too quixotically interpretive to be useful as a reference work for scholars, and the organization of Fossier's sections is too inchoate to make it suitable as a textbook. Better treatments of all of these subjects already exist in English, a fact that renders the decision to translate rather surprising.

DAVID NICHOLAS
University of Nebraska

MODERN EUROPE

E. J. HOBBSAWM. *The Age of Empire, 1875–1914*. London: Weidenfeld and Nicolson. 1987. Pp. 404. £16.95.

The Age of Empire is the third and last panel of the triptych Eric J. Hobsbawm has dedicated to the long nineteenth century: his *Age of Revolution* (1962) ran from 1789 and its preliminaries to 1848; *The Age of Capitalism* (1975) carried the story to 1875; the present volume closes it at 1914. Each volume is idiosyncratic, copious, colorful, and readable; the present book even more so than its predecessors. This is no wonder. Hobsbawm has reached home territory; he writes about the world of his parents, uncles, friends, the source from which his own world and ours would storm and flood on the twentieth century.

One problem this creates is that we think we know what to expect. Even day-trip readers may nod over the *déjà-lu*, and specialists, jaded of eye or palate, tend to become finicky and demanding. Hobsbawm triumphs over banality by dint of thoughtfulness, wit, and charm. The expected is there: an economy that changed gear (again), the intrusion of mass democracy into political systems that called it into being but found it awkward to accommodate, the consequent “uncertainties of the bourgeoisie” (title of chapter 7) and the compensating certainties of nationalism and socialism, new currents that mobilized workers, women, scientists, and artists, undermining existing orders that World War I would certify as inadequate. First and last, in an age when exploration, once “discovery,” became a sporting exploit, there is imperialism—subject of fashion even more than of interest—and the revolutions that it helped spur.

All this, although beautifully written and maintaining a convincing balance between continuities and discontinuities, would not sustain an initiate’s interest. What makes the reading moreish is not the opportunity to hear again the things we heard before but the personal remarks the author generously sprinkles on our tour.

“If political philosophers dreaded the emergence of the masses, salesmen hailed it” (p. 49). And a whole chapter demonstrates how economic concentration, corporate organization, and scientific management both encouraged and exploited mass consumption. “The age of democratization . . . turned into the era of political hypocrisy, or rather duplicity” (p. 88). That is why hustings, no longer controlled by decent oligarchical negotiation, would henceforth be manipulated from smoke-filled rooms, pending the advent of no-smoking studios. The nation became “the new civic religion of states” (p. 149), its catechism

inculcated in classrooms, than which, until the advent of television, Hobsbawm declares, there was no better medium of secular propaganda. In the service of the nation, *fin-de-siècle* nationalism, the substitute religion of anticlerical politics, stifled local dialects (“languages without an army and police” [p. 156]), encouraged newly invented traditions, and became especially popular “when drunk as a cocktail” (p. 163)—that is when mixed with socialism, science, or art.

Imperialism, of course, is another aspect of nationalism. And Hobsbawm’s reference to South America, “then an informal part of the British Empire” (p. 2), evokes my Cambridge tutor’s attack on a visiting American historian for the United States’ unwarranted poaching on “our” preserves.

On the sociocultural front Hobsbawm keeps pointing to the seeds of change carried by bourgeois, capitalist, liberal principles: seeds of democracy, of anti-imperialism and anticapitalism, of the emancipation of women, workers, and the sexually marginal but also of cultural ferment, stimulation, perturbation, and diversification. One fall-out of flagging nineteenth-century liberalism was the greater accessibility of once-elite activities, in free libraries and museums but also in mass-produced phonographs, cameras, reproductions, cheap editions, and the cinema. Another was the reaction of avant-gardes, consistently “marching in directions the main army of the public was neither willing nor able to follow” (p. 235). Yet another were the urban equivalents of folk art—flamenco, tango, jazz—that “owed nothing of substance to bourgeois culture” (p. 237) but everything to the cultural slumming of fashionables and intellectuals greedy for novelty.

One could go on, but, far better, read the book. One does not have to agree with everything in it to find it substantial, fun, and, as prices go these days, not overpriced.

EUGEN WEBER
*University of California,
Los Angeles*

ULRICH WENGENROTH. *Unternehmensstrategien und technischer Fortschritt: Die deutsche und die britische Stahlindustrie 1865–1895*. (Veröffentlichungen des Deutschen Historischen Instituts London, number 17.) Göttingen: Vandenhoeck und Ruprecht. 1986. Pp. 316. DM 68.

During the last decade of the nineteenth century, imperial Germany began successfully to challenge the historic preeminence of Great Britain in several key branches of industry. The German challenge was particularly significant in the produc-

tion of steel. Steel had become the primary new material of the second industrial revolution, and the impressive output of Germany's mills signaled the growth of German power. Moreover, the fact that Germany's steel production rested almost entirely on technological innovations pioneered in Great Britain suggested a failure of British initiative. Contemporaries soon detected a British "entrepreneurial decline," and economic historians subsequently debated its causes, contrasting it with the more successful German model of industrial development.

In joining the debate, Ulrich Wengenroth has decided above all to analyze the actual operations of the German and British steel industry between the 1870s and 1890s. His analysis of fourteen representative German and British mills allows him to focus on two central issues. First, he identifies, traces, and compares the technical stages and levels of the two nations' mills with considerable accuracy. And, second, he examines the business strategies pursued by the German and British industries during a turbulent period of economic expansion and contraction. His findings are most revealing.

After carefully tracing the history of the adoption and nonadoption of the new techniques of steel production (the Bessemer, Thomas, and Siemens-Martin processes) by both the German and British mills, Wengenroth essentially dismisses the idea that technological factors were responsible for the German success or any British lag. Technical innovations could not be kept secret, technology transfer flourished, and German and British technical levels remained roughly the same throughout the period 1865–95.

The key to the eventual German success was that Germany's industrialists, hard-pressed by the economic crisis of the 1870s, managed to receive not only strong financial support from banks but also tariff protection from the state. In addition the formation of cartels had already begun, being the result of economic crisis as well. The three factors—bank and state support, and particularly cartelization—permitted Germany's steel industry to survive, to organize itself effectively, and to continue to operate largely without regard to world market mechanisms. German steel even resorted to the practice of "dumping."

By the 1890s the German steel industry had evolved into a formidable collective power on the international marketplace—a *kollektive Marktmacht*. As such, it decisively surpassed Great Britain in steel exports, at least after 1909. If there was a British failure, it was that Britain's steel industry did not transform itself into a similar *kollektive Marktmacht*. That British steel shunned this course was due neither to any "entrepreneurial decline"

nor to the unwillingness of British industrialists to cartelize momentarily, to receive governmental favors, and to practice dumping. Rather, the British steel industry, not having been formed amid crisis, chose to adhere to what Wengenroth calls a system of "entrepreneurial autonomy" chiefly because this system had paid off in terms of substantial profits for a long period of time. By contrast, as the prominent railroad and steel magnate I. L. Bell testified before the Royal Commission in 1884, German and Belgian steel enterprises were no more than "moderately profitable" (p. 293). The German model, therefore, seemed hardly worthy of imitation—at least during the period examined by Wengenroth.

Regrettably, the author could not examine the British response to Germany's *kollektive Marktmacht* once its successes had become manifest. Nor does he elaborate on the complex politics involved in the evolution of the German model; in this regard, a chapter on the German "politics of steel" would have helped. Nevertheless, Wengenroth's careful and suggestive study persuasively challenges the conventional wisdom of economic historians who must now contend with his findings. The book, not surprisingly, won the Kellermann prize of the Association of (West) German Engineers.

MANFRED J. ENSLE
Colorado State University

BRIAN PEARCE. *How Haig Saved Lenin*. Foreword by EVAN MAWDSLEY. New York: St. Martin's. 1987. Pp. xii, 138. \$29.95.

Perhaps it is true that historians of the Russian revolution and those of World War I have performed their function in "self-contained worlds," as Evan Mawdsley maintains in his foreword to this short but provocative monograph (p. x). Brian Pearce has sought to bridge that alleged gap with a collection of related essays centering on Russian, German, and British relations from the Treaty of Brest-Litovsk to the armistice in November 1918. The symbolic title sums up his major thesis: that the British offensive at Amiens in August under the command of General Douglas Haig was decisive in the military collapse of the Central Powers and the preservation of Bolshevik Russia from German domination. Subsidiary themes include a debunking of the "Soviet myth" that Communist propaganda significantly weakened the German army and an apologia for Allied military intervention in Russia.

Despite an insistent "revisionist" interpretation and a sustained aura of constant discovery, the subject matter hardly involves unexplored terrain. A host of scholars have seemingly exhausted the

nuances of Soviet diplomacy in 1918, none more thoroughly than Richard Debo in his *Revolution and Survival* (1979), as Pearce obliquely concedes in his preface. Nonetheless, obscure sources have been used along with more obvious ones, and the documentation, including a lengthy if necessarily selective bibliography, is impressive. The prose is lively enough to engage a wide audience, yet few concessions are made to the uninitiated. What, for example, was a Czechoslovak Legion doing on Russian soil? And who, even sophisticated readers might wonder, were such unidentified figures as Antonov-Ovseyenko, Hanecki-Fürstenburg, and von Hutier?

The author's premises are those of a British patriot, and he assumes that a German victory would have been a disaster, not only for Lenin's Russia but for Europe as a whole. So it might. But if historians may legitimately pursue "what if" scenarios, then surely it is relevant to suggest that the empire of "Kaiser Bill" was a model of enlightened statecraft compared to the genocidal barbarism of Nazi Germany. Could Adolf Hitler and World War II have been avoided if the British had managed to lose at Amiens?

ROBERT D. WARTH
University of Kentucky

A. L. ROWSE. *Court and Country: Studies in Tudor Social History*. Athens: University of Georgia Press. 1987. Pp. x, 310. \$24.95.

This volume is a collection of biographical sketches of more or less prominent sixteenth-century individuals loosely connected by their largely West Country origins and by the court and country of the title. The individuals range from members of politically important noble families such as Honor Grenville, Lady Lisle and Edward Courtenay, earl of Devon, to a minor Cornish country gentleman, William Carnsew. They also include Sir Peter Carew, an adventurer in pursuit of family fortune in Ireland; Richard Topcliffe, an official charged with rooting out Catholic opponents of Elizabethan government; Henry Cuffe, secretary to the earl of Essex, who was executed in the aftermath of his master's rebellion in 1601; Richard Carew the Cornish antiquary; and finally, Sir Richard Hawkins, member of the famous Devon line of seafarers.

The collection's strengths are those familiar to readers of A. L. Rowse's other works: the author's vigorous prose, his immersion in the evidence, and, above all, his evident love of the period and its personalities. Rowse's views are stated openly and sometimes bluntly. He is nostalgic for the Elizabethan past and identifies with an idealized

version of a *via media* lying between the extremes of Tridentine Catholicism and iconoclastic Puritanism. While admiring the work of G. M. Trevelyan, "our finest contemporary historian" (p. 136), Rowse is no whig. Somewhere, something went wrong. He quotes with approval the brass memorializing a Cornish gentleman: "a lover of his country, friendly to his neighbours, liberal to the poor." Then follows a stinging comparison with the present: "Such were the values the Elizabethans esteemed and promulgated—of more worth than those that prevail in our shiftless society" (p. 146).

Rowse echoes Elizabethan condemnations of Spanish Catholics as merciless and bloodthirsty persecutors. The result is exaggeration of the activities of the Council of Troubles in the Netherlands (p. 183). He repeats the old Protestant canard that the papal bull deposing Elizabeth preceded and led to the Rebellion of the Northern Earls (p. 184). Many other groups come under Rowse's guns, including Puritans, Germans, and the Irish. He is also hostile to any political attempt to court popular favor—the people are the vulgar mob. Eventually, one tires of Rowse's sweeping asides and wishes that he had heeded his own stricture and dealt "in more concrete observations, away from the dubiousness of generalisation" (p. 261).

Over the past two decades a great deal of scholarship has been devoted to the court and country theme in Tudor and Stuart history. One now expects that a book on court and country will deal with the competing political and social obligations to the crown, on the one hand, and to the local county community, on the other. But Rowse treats court and country as separate places with little interconnection. The court is the, often risky, place of politics and personal ambition, and the country is the place of family origins, domestic routine, and estate management, devoid of politics. A related problem is the volume's old-fashioned view of social history as the miscellaneous oddments of dress, food, and daily life quite separate from politics, religion, and other important matters. For example, in the chapter on the Lisles, discussions of the political and administrative problems of the Lord Deputyship of Calais alternate with unrelated descriptive passages on the contents of the Lisle correspondence relating to domestic life.

Despite these criticisms, the volume is well worth reading. It is full of vigor and presents a forcefully stated and detailed view of one dimension of Tudor society.

BUCHANAN SHARP
University of California,
Santa Cruz

ESTHER S. COPE. *Politics without Parliaments, 1629–1640*. Winchester, Mass.: Allen and Unwin. 1987. Pp. xiii, 252. \$29.95.

The political history of the period from 1629 to 1640, when Charles I ruled without Parliament, has long remained obscure. Without the records of a national political assembly, it has been particularly difficult for historians to identify the political concerns of the nation and measure the strength of opposition to royal policy. In this amply researched book, Esther S. Cope has made effective use of letters, diaries, petitions, and judicial proceedings to gauge the responses of local leaders to such developments as the introduction of Laudian forms of worship, the raising of revenue for defense, and the extension of the royal forests. The book observes a number of self-imposed limits. It makes no attempt to provide a narrative history of the 1630s, and, despite frequent references to "the English" and "people," it is almost entirely concerned with "the king's leading subjects" (p. 7). It deals with popular politics only to the extent that the lower classes influenced the actions of the elite. Although the main geographical focus of the book is the localities, it is not a study of local politics as such. Its main purpose is to fill a gap in the history of English national politics.

Cope is at her best at the beginning of the book, when she describes the aftermath of the dissolution of Parliament in 1629, and at the end, when she shows how the conflicts with Scotland in 1639 and 1640 led local elites to see connections between previously isolated grievances, to complain about the long absence of Parliament, and to refuse the king's requests for military and financial aid. The intervening, thematic chapters on religion, defense, and "projects" (such as distraint of knighthood) are less satisfactory, mainly because they lack clear direction. They reveal a considerable amount of "grumbling" over a variety of issues but leave the reader with little more than a sense that grievances were accumulating. Cope argues in the conclusion that "religion dominated politics and gave it continuity" during these years (p. 213), but this promising theme is not developed in the thematic chapters. Nor do these chapters give much of a sense of chronological development, except to the extent that they look ahead to the climactic events of 1639 and 1640.

Cope tries to steer a middle course between the whig view that England was on a "high road" to civil war during the entire decade and the claim of some revisionists that the period was one of relative political harmony. She presents these years, therefore, as a time of considerable discontent but not of revolutionary sentiment. The local leaders who demanded redress of grievances are depicted

as loyal and conservative men whose conduct Charles interpreted as a challenge to his rule; their actions "were radical because the king made them so" (p. 199). This is a balanced and reasonable characterization of these men's politics, one with which most whigs and revisionists could agree. Whether the first Bishops' War of 1639 was the great watershed in the political history of this period is more open to question. By Cope's own admission, there was a striking difference between the situation in December 1639 and the summer of 1640, after the dissolution of the Short Parliament, and it was not until September 1640 that English politics were "transformed" (p. 165). On the other hand, one could make a case, using some of Cope's evidence, that 1637 marked as much of a turning point as either 1639 or 1640. In any event, historians of this period will find this book valuable, both for presenting abundant evidence of local discontent in the 1630s and for illuminating the political events of 1639 and 1640.

BRIAN P. LEVACK
University of Texas

MARK GOULD. *Revolution in the Development of Capitalism: The Coming of the English Revolution*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xxvii, 508. Cloth \$48.00, paper \$14.95.

This is an ambitious but frustrating book. Mark Gould has constructed a social theory of breathtaking complexity, but his theoretical ambition is not matched by expository clarity. Drawing inspiration from (among others) John Maynard Keynes, Jean Piaget, Talcott Parsons, Karl Marx, and Max Weber, Gould presents a three-dimensional (structural, functional, developmental) construct for understanding social life. Sociologists must judge Gould's theoretical contributions. Certainly, his catholicity highlights connections worthy of serious consideration and rightly reemphasizes the discreteness of events too often conflated. But, for historians, the slog through Gould's socio-speak will yield only minor rewards.

As far as I can reconstruct it, Gould argues that societal strain results from imbalances (inflationary or deflationary) in social media (money, power, influence, value commitment) exchanged between four subsystems (roughly, polity, economy, community, and ideals). Such strains neutralize conventional political loyalties, and, when a forum exists in which to express such doubts, however illegitimately, strains can become disorder. This disruption will occur where the social strain is greatest (role relationships, collectivities, norms, or values). If alternative ideals can legiti-

mize the disorder, it can become the catalyst for a transformation that moves a society to a new stage of social development.

Gould interprets the English revolution as such a transformation. Seeing a dissonance between the economic and the political structures of early seventeenth-century England, he argues that Charles I aggravated that dissonance, that Parliament provided the opportunity to express discontent, and that Puritanism offered an authoritative structure alternative to the court. Gould identifies and names four revolutions in the 1640s: REVOF, a contest over roles within the established political structure, precipitated by the Bishops' Wars, caused by strain over facilities, and focused in the Long Parliament; REVOG, a contest over the ideals of the polity, precipitated by the Irish Rebellion, caused by strain over goals, and focused in the Parliamentary party; REVON, a contest over the structure of governance, precipitated by the end of the second Civil War, caused by strain over norms, and focused in the New Model Army; and REVOV, a contest over the nature of the polity itself, precipitated by the success of the Civil War, caused by strain over values, and focused in the Leveller party. Because Charles II accepted only the first revolution (REVOF), 1660 solved very little. Only after William and Mary institutionalized the second revolution (REVOG) did harmony between economy and politics return, making possible England's transition from manufacturing capitalism to machine capitalism.

Seventeenth-century specialists will find little new here. Gould reworks familiar, now contested categories (a country opposition, a revolutionary Puritanism, a rising merchant class), but he refuses to engage historiographically and merely recapitulates such ideas without reinvigorating them. Although it is good to see the revolution discussed as more than an adjunct to the Civil War, Gould's aggregated (rather than integrated) model seems not only mechanical but also narrowly political. Moreover, sections of his argument are incomplete. He tries to build a viably dynamic model for the 1640s without a detailed analysis of the 1650s. And he makes radicalism in religion critical yet refuses to confront Puritanism's quite different effects outside of England, the conservatism of religious independents under Oliver Cromwell, or the quietism of the Restoration era. Gould reminds us how important it is to understand the past holistically and dynamically, but his results show why that is so difficult. He begins his book with a quotation from Virginia Woolf, "Never pretend that the things you haven't got are not worth having." Woolf is right, of course, but they are not worth reifying either.

CYNTHIA HERRUP
Duke University

MAURICE ASHLEY. *Charles I and Oliver Cromwell: A Study in Contrasts and Comparisons*. New York: Methuen. 1987. Pp. 243. £12.95.

Most of the readers of this journal are, like the reviewers, academics, and we write and review mostly for each other. The writing of history has become so specialized that we sometimes have difficulty deciding what to think about works aimed at that mythical creature, surely the ideal Ideal Type, the "intelligent lay reader" of history. Normally glib in our campus habitats, our tongues knot up when we are asked to address her or him. Maurice Ashley has written twenty-eight books, most of them aimed at such readers, and this is his latest contribution to the genre. Six of his books have Oliver Cromwell's name in the title, and this essay in comparing and contrasting Cromwell and Charles I is therefore the product of a long immersion in the history of Caroline and Interregnum England. It is in many respects a useful introduction to the politics and personalities of Charles I and Cromwell. The king and his successor are compared in terms of their approach to the making of foreign policy and war and the handling of parliaments and courtiers. Ashley offers a number of sensible judgments. He rightly points out, for example, that Charles's fen-draining schemes were based as much on concern to prevent flooding as on desire to make a profit and that Cromwell supported such undertakings under some circumstances, despite his well-known support for the commoners in 1637–38. Ashley is properly skeptical of speculations about whether Cromwell's "lost years" included military service overseas and of scandalous stories about Cromwell's relations with Frances Lambert and Elizabeth Murray.

Ashley has unearthed no new sources, and he presents no facts that will surprise specialists. For Charles I, Ashley relies heavily on the two most recent biographies, those by Charles Carlton and Pauline Gregg. He has read widely in recent scholarship, but in places he seems not to have appreciated its import fully. For example he quotes James I's well-known absolutist statements without noticing how heavily James qualified them, especially in his speech to Parliament in 1610 in which he sharply distinguished between the power of kings in early as opposed to "settled kingdoms." Without seeming to be aware of the way it contradicts his descriptions of a more bump-tious and aggressive House of Commons, Ashley quotes G. R. Elton's recent denial that the Stuarts were in fact facing such a body. Ashley seems quite unaware of the implications of Patrick Collinson's account of the Jacobean church, nor does he explain why he rejects Nicholas Tyacke's thesis

that the Laudians were the real innovators in early Stuart religion. It is in just such matters that Ashley could have performed the valuable service of introducing readers to the exciting ferment that revisionist scholars have brought to early Stuart history. If he had included more detailed discussions of his agreements and disagreements with them, the nonspecialist reader would gain much more from this book.

SEARS MCGEE
University of California,
Santa Barbara

LEO MILLER. *John Milton and the Oldenburg Safeguard: New Light on Milton and His Friends in the Commonwealth from the Diaries and Letters of Hermann Mylius, Agonist in the Early History of Modern Diplomacy*. New York: Loewenthal. 1985. Pp. xiii, 370. \$45.00.

In August 1651 Hermann Mylius, ambassador from Oldenburg, arrived in London to propose a neutrality treaty or "Safeguard." Mylius's diaries are the main source for this book by Leo Miller. Extracts from the diaries are given in the original German, together with Mylius's correspondence in Latin, followed by translation into English and comment from the author, so that the reader is given a coherent picture of the negotiations. This study brings out fully the important role that Milton, as secretary for foreign languages to the Council of State, played in the success of the mission because of his contacts with leading men in the English government.

As representative of a petty German principality, Mylius found himself brushed aside while the politicians dealt with more urgent matters. The count of Oldenburg was also unjustly suspected of giving aid to Charles II, whose forces had been defeated at Worcester a few days after Mylius came to London. Negotiations dragged on for nearly six months, and, at one point, vital documents were lost through the carelessness of English officials. Understandably, there is sometimes a note of almost comic desperation in Mylius's letters, as when he begged Milton to "get it [the signing of the treaty] done so that finally, away from your embrace, with mind and brow relaxed, I may sail away from here to the lawful joys of my familiar roof and bed, and restore the loss suffered in my absence by the sharer of my limbs and labor" (p. 196). In this book the resourceful, persistent, rather likable personality of Mylius emerges far more clearly than that of Milton.

The diaries provide some incidents of human and biographical interest. Mylius greets Oliver Cromwell visiting the park with his wife to see the

animals. He hears from the Puritan preacher Hugh Peter stories of his sojourn in New England and is entertained by him with "delicacies and Indian treats" (p. 108). Several references are made to Milton's eye inflammation and headaches as total blindness descended on him. Mylius discusses with him the constitution of the English republic and Milton's "drubbing of Salmasius" (p. 128), his recently published *Pro Populo Anglicano Defensio*, the devastating reply to *Defensio Regia* by Claudius Salmasius. In one instance Miller sees in Mylius's account a prophetic recognition of Milton's greatness that is not clearly confirmed by the diaries. They do, however, provide evidence of Milton's official activities and refute the claim of earlier writers that he played a part in the deliberations of the Council of State. His role was primarily that of a translator.

Does the acquisition of such information justify the tedium of reading Mylius's numerous letters with their flowery and effusive humanist style—"Jewel of men, star of friends," and so on (p. 109)? The answer is, probably only for the most avid devotees of Milton. Yet it is a tribute to the man that even so minor an episode in his life should inspire such careful scholarship as this, based on research in England, continental Europe, and the United States.

AMOS C. MILLER
University of Houston

JEREMY BLACK. *The English Press in the Eighteenth Century*. Philadelphia: University of Pennsylvania Press. 1987. Pp. xv, 321. \$44.95.

Among the most noteworthy developments in eighteenth-century England was the emergence of the popular press in the form of pamphlets, periodicals, and newspapers. The output of printing presses had always concerned government; by the end of the eighteenth century the press had virtually become a part of the system of governing; a fourth estate possessing a special liberty vigorously exercised. Jeremy Black describes this press as it appears to modern scholars. The problems involved in such a survey are daunting; most press historians have limited their work to particular periods or periodicals. Black deserves great credit for his daring and success in putting the whole in focus, presenting a general view that is topical in organization and pragmatic in approach. He deals with the provincial press as deftly as with the dominant London press, and he is as concerned with ownership and production as with politics and the press. His reading in eighteenth-century newspapers and twentieth-century monographs has been voracious.

Black's thematic study opens with the lapse of prior censorship in 1695 and the rise of the newspaper. The reign of Queen Anne saw much activity—and many failures. Augustan politicians quickly learned how to use and abuse the press; their successors learned to tax it but not how to control it. Once “the liberty of the press” was unleashed, it would never be chained as before.

The contents of the press defy statistical analysis. Editors' and contributors' tastes varied as much as readers' prejudices. The press provided something for everyone: politics (often at the behest of great men), business (shipping and farming for the middling sort), crime and sport (the real opiate of the masses), and moral or ethical instruction (for the betterment of all).

Although noting that a full-scale reexamination of the relationship between the press and politics is “urgently required” (p. 113), Black insists that the press be viewed as part of the system, not separate from it. There was a general belief in the efficacy of using the press, but neither subsidy, threat, nor legal prosecution could control it. Efforts in that direction had “relatively little direct political impact,” yet attempts at control had “a wider effect that was both difficult to assess and politically to exploit” (p. 143).

Black's chapter on the press and Europe is unique. The English press was proudly xenophobic, often ill-informed, lacking in regular or trustworthy sources of information, but its reporting of foreign news helped break down English parochialism. In the area of moral and religious improvement, the press was “preceptive rather than persuasive” (p. 246). Methodists were mocked; issues such as slavery, drink, cruel blood sports discussed; the press was generally “sympathetic to popular distress but opposed to popular action” (p. 272).

Black questions whether a changing press was altering society. Although there were visible changes in newspapers prior to 1725 and later changes in headlines, leading, and editorializing, except for the reporting of parliamentary debates the newspaper business was a conservative trade. If it gained in readership, it offered little but more of the same reading. Its “fundamental continuity and . . . conservatism” were characteristic of the century (p. 291). It became the accepted means of conducting political debate, recognized its own role as “both an agent and a participant” in that process (p. 294), but was “substantially less influential than is commonly believed” (p. 306).

Black's extensive use of quotations from a wide variety of newspapers provides a delightful taste of eighteenth-century journalism. He does not provide (nor was it his intent) a narrative of those events in which the press figured prominently. His

rejection of simplistic whiggish interpretation will doubtless be criticized, but he raises questions that will make informed readers ponder. If he does not provide absolute answers, he challenges others to further research in the history of the English press.

ROBERT R. REA
Auburn University

MARK PHILP. *Godwin's Political Justice*. Ithaca: Cornell University Press. 1986. Pp. x, 278. \$29.95.

With the publication of *An Enquiry Concerning Political Justice* in 1793 William Godwin became, for a few years, Britain's leading philosopher. By 1800 he had lost public favor, and he spent his last thirty-six years in poverty. His book retained its fame because Godwin had concluded that with the increased use of reason government would slowly fade away. This theory made *Political Justice* a classic work of anarchism.

Godwin further contended that as humankind perfected itself property, marriage, disease, sleep, and even death would vanish from existence. Why a philosophy espousing such absurdities should win such initial favor and Godwin's sudden fame and subsequent downfall have interested intellectual historians ever since. An entire school of Godwin scholarship has developed to explain the philosopher and his book.

Mark Philp presents a new analysis of Godwin from 1790 to 1800 and of all three editions of *Political Justice*. He rejects explanations that Godwin's inspiration came from the French revolution, that he was an early Utilitarian, or that his views were similar, in a confused, middle-class fashion, to those of the radical artisans of the period, as E. P. Thompson has argued.

According to Philp, Godwin's philosophy sprang directly from the heritage of British religious dissent. A former dissenting minister himself, as was his father, Godwin wrote in a period when the Test and Corporations Acts were still in force. The heart of Godwin's philosophy, Philp concludes, was the necessity for each person to exercise fully his or her private judgment. Continuation of government would prevent full and free use of private judgment.

By 1790 Godwin became a member of a social circle of radical and rational dissenters within which he was able to work out the ideas found in *Political Justice*. To document his conclusion, Philp has used Godwin's diaries to chart his contacts within this radical circle.

The reason *Political Justice* was so warmly received, according to Philp, was the presence in Britain of a community of middle-class radical and

rational dissenters. The British radicals, anticipating government reform at home, strongly supported the early stages of the French revolution. The course of the French revolution in 1793 and after and the commencement of war between Britain and France at the time of the publication of *Political Justice*, Philp claims, were key factors in altering the later reception of Godwin's book. Government repression effectively destroyed British radicalism by the end of the century. Godwin's radical circle fell apart, and the middle class became conservative.

Godwin's experiences after 1793 explain his revisions in the 1796 and 1798 editions of *Political Justice*. Support from his radical associates declined. When he married the radical feminist, Mary Wollstonecraft, in 1797, his own reputation suffered.

Philp has produced an impressive work of intellectual detection. By correlating *Political Justice* with Godwin's personal life through all three editions, he builds a convincing case for his new interpretation. Written for Godwin scholars, Philp's work is a major contribution to British intellectual history.

JOHN P. CLARK
Fitchburg State College

STEWART ANGAS WEAVER. *John Fielden and the Politics of Popular Radicalism, 1832-1847*. New York: Oxford University Press. 1987. Pp. ix, 320. \$62.00.

In this book on John Fielden's public life during the fifteen years he represented the borough of Oldham in the House of Commons, Stewart Angus Weaver argues that to see Fielden as a Tory Radical—"to the extent [Tory Radicalism] ever existed" (p. 11)—is to misunderstand both him and nineteenth-century popular politics, in which Fielden was "a crucial mediator" (p. 109). Weaver discusses Fielden's potpourri of popular attitudes in chapters ordered topically rather than chronologically: admiration for William Cobbett, advocacy of paper money, national regeneration, sympathy for the handloom weavers, support for factory legislation, opposition to the New Poor Law, promotion of the Charter and manhood (sometimes universal) suffrage, and, finally, sponsoring of the 1847 Ten Hours Act. There is consequently some overlapping and confusion, and Fielden emerges as a somewhat eccentric self-made man, a manufacturer unable "to turn a significant profit" (p. 141), who organized strikes among his own workers, who turned three thousand out of work to protest the New Poor Law, yet

whose "single commitment was to the popular constituency in whatever form it chose to express itself" (p. 16).

Weaver sees only "one popular movement during the early Victorian period, based on a simple belief in the political rights of labour" (p. 6); at its center is Fielden's political career. It is not easy to reconcile the conclusions this thesis generates. For example, on the opposition to the New Poor Law, Weaver writes: "The anti-Poor Law movement lasted only two years, largely because the very logic of its argument . . . necessitated the creation of a new political movement: Chartism" (pp. 11-12); "after 1836, opposition to the Poor Law was the dominant theme of the radical movement" proposing "a real alternative based on new conceptions of welfare relief" (p. 163); "after 1836, factory laws and poor laws were indistinguishable issues" (p. 163); "on Hartshead Moor [May 15, 1837] the anti-Poor Law movement came of age" (p. 175); "these were the months [early 1838] in which the anti-Poor Law movement was evolving indistinguishably into Chartism" (p. 187).

But the "indistinguishable" Ten Hours' movement was quite distinguishable and very strong, if not dominant, in the north after 1838, by which time Chartism is said to have "fully sapped the strength of the anti-Poor Law movement" (p. 176). Fielden's own opposition to the Poor Law in Parliament and in Todmorden continued, as Weaver's account makes clear, after the summer of 1839 when his "single-minded commitment to the People's Charter . . . sincere but nevertheless fleeting," had come to an end (p. 216). (This last description of Fielden's Chartism is not further removed from the oxymoronic by an earlier claim that there was "then, no question of Fielden's becoming a Chartist; he had to a great extent been one all of his life" [p. 210].)

Weaver's insistence on Fielden as "the political embodiment of working-class aspirations" (p. 16) leads to some unusual characterizations of other participants. Jeremy Bentham and John Locke head a list of Unitarians committed to reform (p. 35); Lord Ashley is characterized as an "extremely self-righteous evangelist" (p. 5); John Bright joins the "Benthamite purists" (p. 297); W. J. Fox is heralded as "a liberal pundit from Norfolk" (p. 259); and "most of the thirty-nine members [of a select committee in 1833] were either Whigs or Liberals, if not outright political economists" (p. 71). It is misleading to write: "To speak of John Fielden as an influence upon Marx would be perhaps too dramatic" (p. 152). There could be no justifiable amount of drama in the matter, as Weaver himself goes on to make clear. The same is true of the remark, "of course the Poor Law was

never completely baffled" (p. 11), indeed not. And it is certainly wrong to say that the Reform Bill "was thrown out by the Lords in September 1831, [and] by the King in May 1832" (p. 51) and to talk of "the defence of the Ten Hours Act" before 1847 (p. 163).

Even without these dubious characterizations and exaggerations, Weaver's attempt to thrust Fielden of Todmorden into "the centre of the century's most turbulent decades" would not be successful (p. 16). He emerges as a superficial thinker of uncertain reliability, to whom justice is not seen to have been done.

ANN ROBSON
University of Toronto

DAVID B. SWINFEN. *Imperial Appeal: The Debate on the Appeal to the Privy Council, 1833–1986*. Wolfeboro, N.H.: Manchester University Press. 1987. Pp. viii, 268. £35.00.

The Judicial Committee of the Privy Council has acted as the court of final appeal for imperial cases since 1833, whereas for the citizen of the United Kingdom the House of Lords has served as the court of last instance. In this division of appellate authority lies the heart of this excellent book that, despite its title, touches on issues of much broader interest. David B. Swinfen does not attempt to chronicle the work of the Judicial Committee nor to analyze the jurisprudence that resulted from its decisions. The primary argument states that the opposition to appeal to London has been political, not juridical. Thus, the gradual elimination of imperial appeal has acted as a useful barometer to changes in the wider sphere of the empire-commonwealth itself.

The judicial work of the Privy Council in its heyday had several goals. The power of judicial review emphasized the imperial doctrine that all colonial legislatures were subordinate and that imperial sovereignty resided only in London. The Judicial Committee could declare invalid colonial legislation repugnant to English law; imperial statute remained supreme until 1931 when the Statute of Westminster ended this function. Imperial appeal was also intended to provide a uniformity of law throughout the empire. The author suggests that this task was impossible because of the diversity of legal systems under the committee's jurisdiction. He also questions whether this objective was well conceived in the first place from the perspective of legal theory. Finally, the symbolic role of imperial appeal, the common law as a golden thread tying different peoples and cultures

together, became in the minds of proponents the most important purpose of all.

In law imperial appeal meant that every subject possessed the right of appeal to the crown for justice. In several countries ethnic or religious minorities regarded this as a fundamental protection against majority oppression. In practice, however, an imperial appeal faced many obstacles. The process was costly and, its opponents argued, favored the wealthy who could afford to prolong litigation. In spite of every effort judgments were frequently years in forthcoming. Critics maintained that judges unfamiliar with local conditions remote from London could not render effective judgments. The single judgment rule meant no dissenting opinions (contrary to the custom of the House of Lords), and different practice marked again the inferiority of national legal systems.

Political movements to abolish the imperial appeal focused inevitably on the issue of status. Could a supranational court survive into an age of nationalist, anti-imperial sentiment? The imperial appeal survived into the twentieth century much longer than expected, but, once Canada and the Irish Free State made a concerted effort to abolish it, its jurisdiction has diminished dramatically in the last half-century.

The author tells this story in a straightforward manner. Although the title would suggest an interest only to legal specialists, the book deserves a wider audience. Scholars pursuing broad themes will find this book a rewarding example of how a minor motif illustrates the larger problems of imperial history. On this basis the book succeeds admirably.

RICHARD A. COSGROVE
University of Arizona

D. C. COLEMAN. *History and the Economic Past: An Account of the Rise and Decline of Economic History in Britain*. New York: Clarendon Press of Oxford University Press. 1987. Pp. 150. \$38.00.

This elegant essay on the origin, nature, and future prospects of economic history in Britain is the result of wide reading and thoughtful reflection. It is both a valuable sketch in the history of economic thought and a plea to his British colleagues for a revitalization of the subject.

D. C. Coleman traces the origin of the study of the economic past to such Scottish Enlightenment figures as John Millar, David Hume, and Adam Smith. Although their work suffered from a Whiggish fascination with liberty, their historical vision incorporated economic, legal, and anthropological matters. They also saw history as a use-

able past and freely employed it to promote various reform schemes. In the tradition of the historical economists of the late nineteenth century, Coleman blames the classical economists, and especially David Ricardo, for neglecting historical concerns in economics in favor of a rigorously deductive method. He also points to the historians, particularly Thomas Macaulay, for avoiding economic issues and concentrating on political and constitutional narrative.

Despite the efforts of such forerunners as Thomas Malthus and Richard Jones and such statisticians as Thomas Tooke and William Newmarch, it was the pioneering work of T. E. Cliffe Leslie that laid the foundation for the academic study of economic history in Britain. Between about 1880 and 1910, the leading historical economists—Arnold Toynbee, W. J. Ashley, William Cunningham, and W. A. S. Hewins—provided a critique of classical and neoclassical economics, attacked the instabilities of *laissez-faire* capitalism, broadened the scope of history beyond past politics, and made room for economic history within the universities. During the same period Alfred Marshall used mathematics, marginal utility analysis, and his considerable authority to transform political economy into economics and to provide it a secure place within the university curriculum. The result was a series of celebrated conflicts over the future of economic studies, the most famous of which were those between Marshall and Cunningham at Cambridge. The echoes of these disputes can be heard to this day. With the notable exceptions of the London School of Economics and the Birmingham Faculty of Commerce, economic history found its early British home in departments of history.

Born out of the reformist tradition of the historical economists, economic history's critical attitude toward society and orthodox economics was continued by John L. Hammond and Barbara Hammond, R. H. Tawney, G. D. H. Cole, and, more recently, by Marxists and neo-Marxists. According to Coleman, a neutralist tradition also emerged, beginning with the work of George Unwin and J. H. Clapham. Coleman defines as neutral those economic historians whose primary goals are not the making of moral judgments and the reform of society. Instead, they seek to apply the basic concepts of neoclassical economics to the study of the past. Although Coleman admits that the neutralists have their own bias, he is clearly more sympathetic to their approach. It is the neutralists who now dominate the profession and oversaw its phenomenal growth between 1946 and the early 1970s. At the height of its popularity, there were fifteen independent departments of economic history in Britain.

For Coleman this separatist victory was bought at a heavy price. Increasingly isolated, the economic historians have neglected both recent innovations in historiography and the abstract and mathematical work of modern economics. While some economic historians have been seduced by social history, others have embraced the model building of the new economic history. The latter Coleman characterizes as past economics rather than history. In their adoption of complex mathematical growth models, the new economic historians have forgotten Marshall's caution that neo-classical economic tools have limited utility for analyzing large problems and long periods. Moreover, historians have found little empirical validity in such work. Coleman also remains skeptical of universal history as practiced, for example, by Fernand Braudel. Thus, ignored by historians and forgotten by economists, economic historians have fallen into the trap that Ashley warned against in 1926 of "cultivating a little garden plot of our own." Further, they do so within an increasingly hostile public climate. The solution, according to Coleman, is for economic historians to abandon their separateness and to maintain close ties with both history and economics departments. Students should be trained in both economics (and therefore in mathematics) and history. Problems tackled should be of a manageable scope, and he is especially hopeful about some of the cooperative work being done in local and business history.

Coleman's essay breaks little new ground as a historical account of the rise of economic history in Britain. Its analysis of its decline is more provocative. It is, however, the most judicious and readable introduction to the subject. Because he is an eminent economic historian, his work is assured of a prominent place in the historiography of British economic history.

GERARD M. KOOT

Southeastern Massachusetts University

ALAN FOX. *History and Heritage: The Social Origins of the British Industrial Relations System*. Boston: Allen and Unwin. 1985. Pp. xiii, 481. \$42.50.

In this important study Alan Fox traces the origins, emergence, persistence, and contemporary crisis of modern Britain's industrial relations system, a set of arrangements "strongly characterized by adversary, win-lose postures, characteristics which, taken together with what are regarded as unusually restrictionist policies among Britain's workforce, are often judged as having contributed to Britain's relative economic decline" (p. vi). Starting in the seventeenth century, British work-

ing people fashioned for themselves an ethos compounded of stubborn individualism, sectional solidarity, and instrumental collectivism. Concurrently, the ruling classes were fashioning for themselves a polity rooted in the rule of law, a limited state, and market capitalism. The outcome—which was not rapid, inevitable, or stress-free—was a “calculated risk” taken in the third quarter of the nineteenth century based on “conceding legal recognition to the unions and the franchise to the urban worker” in the hope that “this would bond the respectable working class even more firmly within the constitution and would consolidate legitimation of the system by giving formal recognition both to an extension of equality before the law and to the pursuit of equality of bargaining power in the labor market” (p. 166). Having survived to the present, the system is now facing its most severe challenge at the hands of Margaret Thatcher. Among other things, Fox has written a tract for the times.

So long as it was supported by mid-Victorian prosperity and, later, imperialism, the system worked. For the upper classes, it “certainly proved to have the hoped-for result of preserving the social fabric” (p. 168). The majority of working people also acquiesced, since “experience had taught them that in the face of greatly superior power it was the best they could do and their own leaders . . . were anxious to assure them that this was so” (p. 274). But stability came at the high price of creating massive barriers to economic modernization. Fox decisively rejects the argument that the trade unions were solely responsible; their reluctance to cooperate “was an expression not of mere perversity but of the postures of arms-length wariness and mistrust generated among both sides by the historical development of the system” (p. 173). Instead, he emphasizes the resistance to modernization of all sectors of British society and, especially, the preference of the middle and upper classes for “a spirit of piecemeal, adaptive reform underpinned by a paternalist fusion of aristocratic noblesse oblige and the evangelical doctrine of service and duty” (p. 227). Notwithstanding two world wars, the system retained a measure of viability until the 1970s when the failure of the Labour party’s attempt to maintain full employment, economic growth, a stable currency, and expanded trade union rights gave way to the openly nonconsensual policies of the Thatcher administration. The book concludes by suggesting two sharply counterposed alternatives for the future: either a new politics in which industrial relations will be “harsh, vengeful and embittered” or an updated reassertion of “traditions that are creative, radical and reveal a love of

country that stops short of bellicose assertiveness against others” (pp. 451–52).

Fox’s thesis is a powerful one, driven, on the one hand, by a more or less Marxist insistence that Britain’s industrial relations system has been a product of class conflict that served to protect and enhance the power and privileges of the elite and, on the other hand, by a Fabian insistence that British workers have never sought the wholesale transformation of society but have only been willing and able to undertake struggles for sectional, economic gains through their unions and incremental political and social reforms, for which the main vehicle in the twentieth century has been the Labour party. I do, however, have two reservations. The first is that his case for continuity is overargued. An equally plausible case can be made for the changes in consciousness brought about by the Industrial Revolution, a concept that Fox uses sparingly indeed. The second is that his explanation for the failure of the British to modernize their economy might be a bit too insular, in view of the recent faltering performance of other advanced industrial economies, most notably that of the United States. But, reservations aside, Fox has succeeded admirably in integrating the history of industrial relations into a broader framework in which state, economy, society, and ideology receive full consideration. As a result, this is a book not just for specialists but for everyone concerned with the past and future of British industrial society.

JOSEPH WHITE
University of Pittsburgh

ALAN FOWLER and TERRY WYKE, editors. *The Barefoot Aristocrats: A History of the Amalgamated Association of Operative Cotton Spinners*. Littleborough, U.K.: George Kelsall. 1987. Pp. xiv, 272. Cloth £12.95, paper £6.95.

This volume is a kind of celebrative history of the Amalgamated Association of Operative Cotton Spinners in the United Kingdom. The volume, edited by Alan Fowler and Terry Wyke, was financed out of the original pension fund of the amalgamation, which folded its tents in 1975 as a result of the demise of the entire cotton industry in the United Kingdom.

The celebrative note is struck in the last paragraph of the book: “As one of the earliest permanently established trade union organizations in the cotton industry [the amalgamation] has played a leading role in laying the basis for the unionization of the rest of the industry. They had also guided the cotton operatives in many of their industrial

struggles and conflicts with Government over issues concerning the industry as a whole. The collective action of the spinners' union through their Amalgamation, enabled them to do battle with the employers and achieve for themselves a position in the cotton spinning and manufacturing industries that was second to none." There seems to be no reason to challenge this somewhat glowing assessment of the amalgamation. At the same time the pages of the volume itself elucidate a few themes that are not exactly consistent with it.

The first theme is a certain rockiness in the establishment and the history of the amalgamation. The organization was continuously buffeted by the fortunes of the cotton trade, constantly forced to face rear-guard battles against wage cuts during times of market glut, and always faced with the menacing prospects of organizations of the manufacturers themselves. One of the key things that amalgamation—or centralization—did for the unions was to provide them with a continuity of membership and resources to tide them over the numerous very rough periods in their history.

The second theme is a kind of discontinuity in the history of the amalgamation. The authors chronicle the history of the organization well, tracing it through the various phases of a struggle for survival, consolidation, and amalgamation and the turning from the politics of the industry to the realm of Labour party politics in the first part of the twentieth century. At the same time there was a process of constant backsliding, localism, and regional conflict within the amalgamation that gave it an existence that was less calm than the summary account would suggest.

The writing is detailed, dry, and thorough, yielding a good blend of institutional and personal history.

NEIL J. SMELSER
*University of California,
Berkeley*

J. M. W. BEAN, editor. *The Political Culture of Modern Britain: Studies in Memory of Stephen Koss*. Foreword by JOHN GROSS. London: Hamish Hamilton; distributed by Columbia University Press, New York. 1987. Pp. xi, 306. \$30.00.

The excellence of the fifteen essays constituting this book does credit to the high regard its authors and editor J. M. W. Bean had for Stephen Koss of Columbia University, to whom the book is dedicated. Koss was a brilliant scholar and a warm and generous person, and his death while in his forties was not only a great loss to scholarship but a blow to his many friends. The respect that these friends

had for Koss informs these fine essays, most of which achieve that blend of historical sagacity and clarity of style that distinguished Koss's many studies of Victorian and Edwardian England.

There is even implicit in nine of these essays a common theme, a theme that runs through Koss's writings, the theme of liberalism triumphant and liberalism frustrated. It forms one of the paradoxes of modern England. With deep roots in Protestantism, the Enlightenment, and emerging classes, liberalism was pervasive and powerful, yet, just as it achieved expanded liberties, a wider suffrage, and pioneering welfare measures, it experienced frustration, conflict, failures, dashed expectations, unwanted illiberalities, unpopularity, and, according to George Dangerfield, a strange death.

In the opening essay on Unitarian journalists, R. K. Webb deals with such liberal triumphs and liberal disappointments. Seldom did liberalism achieve a purer form than with the Victorian Unitarians' blend of ethical Christianity, enlightened reason, and middle-class individualism, but, although it made their journalism distinguished and raised hopes of educating a nation to reason and virtue, these hopes and the circulation of their press faded before the cheap, sensationalist, jingoist, yellow journalism of Lord Northcliffe.

Peter Marsh also deals with Unitarianism and liberalism, the Unitarian liberalism of Joseph Chamberlain. In the general election of 1874 the young Chamberlain brought forth his radical liberalism in his effort to become M.P. for Sheffield, only to lose as his liberalism was rejected by a working class passionate for beer and by Nonconformists zealous for theological orthodoxy. Once more a pervasive liberalism meets defeat and frustration, a pattern that again emerges in Peter Clarke's incisive reading of Herbert Asquith, in John Grigg's probing analysis of David Lloyd George on proportional representation, and in Paul Addison's measured review of the politics of Winston Churchill. Never was liberalism more powerful than from 1906 to 1914, when Asquith, Lloyd George, and Churchill carried out the New Liberalism with its Peoples Budget, reform of the House of Lords, National Insurance Act, and Home Rule for Ireland, and never was its collapse greater than when the strains of war, Lloyd George's ambitions, and Asquith's vanities eviscerated the Liberal party.

Pervasive, powerful liberalism even inspired British policy toward India, but haltingly, evasively, timidly. Ainslee Embree's "Pledge to India: The Liberal Experiment, 1885–1909," and Hugh Tinker's "British Liberalism and India, 1917–1945" underline what Koss argued in his

John Morley at the India Office (1969), namely, that the liberals could not square their ideals of liberty, self-government, and nationalism with the complexities of a subcontinent full of religious, ethnic, and political diversity. But, although both essays depict the frustration, hesitancy, short-sightedness, procrastinations, and blunders in imposing Western liberalism on an India short on liberal traditions, liberalism defined and still in part defines India's future.

Much of the pervasive power of liberalism came from its optimism about the future, its rational outlook, and its compassion for ordinary people, three qualities dealt with by Fred Leventhal, Janet Oppenheim, and Peter Stansky. For Leventhal it was an optimism often carried to the point of naiveté that in the 1920s colored the Left's rosy picture of Soviet Communism; for Oppenheim it was the emergence of a more rational approach that led late Victorians to take a moral liberal view of mental illness; and for Stansky it was the sculptor Henry More's compassion for ordinary people that distinguished his superb drawings of Londoners who, during the Blitz, spent night after night in underground stations.

Optimism, rationalism, and compassion are outlooks that, along with the goals of freedom, democracy, and equality can inspire great movements and influence the course of history. But they also raise expectations and challenges beyond the likings and capacities of many men and women and as a consequence bring disappointments and frustrations such as those that, in the reigns of Victoria and Edward, added to the complexity of British history, a complexity that these fifteen essays and the works of Stephen Koss do so much to clarify.

DAVID ROBERTS
Dartmouth College

BERNARD LIGHTMAN. *The Origins of Agnosticism: Victorian Unbelief and the Limits of Knowledge*. Baltimore: Johns Hopkins University Press. 1987. Pp. x, 249. \$29.50.

In recent scholarship the Victorian war between science and religion has lost much of its aggression, and Bernard Lightman's study is designed as a further contribution to the cessation of hostilities. The agnosticism he examines was not a deliberate antireligious challenge to Christianity, provoked by evolutionary theory or biblical criticism. It was inspired instead, Lightman argues, by Kantian epistemological concerns, conveyed to mid-Victorian England largely through the works of Sir William Hamilton and Henry Longueville

Mansel. The five agnostics on whom Lightman focuses attention—Herbert Spencer, Thomas Henry Huxley, John Tyndall, Leslie Stephen, and William Kingdon Clifford—embraced the Kantian view that the structure of the mind imposes its own patterns on human perception as a cognitive barrier. Their agnosticism was molded, Lightman insists, in the context of contemporary theological controversy "over the issue of God's knowability" (p. 31), not by the desire to eviscerate religion on behalf of science. He carefully distinguishes the agnosticism of these men from other forms of British and Continental "unbelief," such as positivism, materialism, and empiricism, and repeatedly asserts that the founders of English agnosticism were sincerely religious men. If they rejected the Christianity of their day, they nonetheless aspired to a purified faith stripped of the dogmas that they considered perversions of Christ's original teaching.

This is a persuasive interpretation of Victorian agnosticism. Although other scholars have identified the religious fervor behind many nineteenth-century expressions of religious doubt, Lightman strikes a new note in emphasizing that Christian thinkers provided the intellectual context for the development of agnosticism. Clearly, his preference is for intellectual history of the old-fashioned sort, which traces particular ideas, through various permutations, from one mind to another. Although he fully acknowledges that Frank M. Turner, Robert M. Young, and Leon S. Jacyna, among others, have raised controversial questions about the social framework in which Victorian agnosticism and scientific naturalism matured, Lightman himself devotes little space to such issues.

In concentrating on the ideas of the leading agnostics, Lightman has deprived himself of the opportunity to speculate more broadly about Victorian discontent with Christianity. He seems unaware that other dissident groups, such as the plebeian spiritualists, shared the agnostics' aversion to such doctrines as biblical infallibility, original sin, eternal punishment, or vicarious sacrifice, while still revering Christ's pristine message and working to create a reformed faith. Even the aspiration to incorporate scientific laws within the new dispensation was not unique to the agnostics.

Surprisingly, Lightman neglects to discuss, and to include in his bibliography, Clifford's short contribution to "A Modern 'Symposium'" in *Nineteenth Century* (April 1877). Lightman's Clifford, having experienced a crisis of faith in his youth, apparently came to accept the implications of radical uncertainty with far greater equanimity than his agnostic colleagues could muster. But this was not the case, as Clifford's comment of 1877

reveals: "We have seen the spring sun shine out of an empty heaven, to light up a soulless earth; we have felt with utter loneliness that the Great Companion is dead."

JANET OPPENHEIM
American University

PAT JALLAND. *Women, Marriage, and Politics, 1860-1914*. New York: Clarendon Press of Oxford University Press. 1986. Pp. xi, 366. \$37.00.

Drawing on the manuscript collections of some fifty political families, Pat Jalland examines the private and social experiences of women who are representative of the political, social, and intellectual elite of Britain between 1860 and 1914. Largely ignored in the past, these women helped shape many of the attitudes and values we characterize as Victorian and thus merit the attention of the historian. Jalland stresses areas of women's lives that have escaped serious investigation—courtship, marriage, childbirth, and spinsterhood—rather than public activities, such as campaigning for suffrage, local governance, participation on royal commissions or in philanthropic work, that have been the subject of a great deal of study. Although this book concerns itself with women who accepted the ideology of separate spheres and "lived within its confines" (p. 17), Jalland also seeks to analyze the roles and attitudes of political wives, daughters, and sisters and their influence on major political figures of the period.

Jalland's impressive scholarship reveals a picture of a stultifying society within which there might nevertheless be great diversity of practice and experience. Her case studies demonstrate that the upbringing of elite children could range from strict discipline and oppressive conformity to lack of supervision and even neglect. Adolescence seems to have been uniformly boring and frustrating for upper-class girls, as the vast majority obtained at home what little formal education they enjoyed. Courtship rituals, on the other hand, might be a source of enormous pleasure for some young women and of horror and dread for others. Upper-class parents no longer directly arranged marriages in the nineteenth century, but social and economic interests continued to determine marriage choices for the Victorian and Edwardian political elite. The London Season operated to ensure that undesirable matches would be avoided. Even here, however, exceptions abounded. Mary Gladstone's unrequited love for A. J. Balfour (to say nothing of her exciting life as her father's political secretary) prevented her from accepting offers of marriage for some fourteen years. She later fell in love with and married a poor clergyman.

Many of the upper- and upper-middle-class women of this study appear to have enjoyed physical and emotional intimacy with their lovers and husbands. Laura Tennant engaged in a great deal of unorthodox sexual behavior with Adolphus (Doll) Liddell, although she never intended to marry him. Molly Bell Trevelyan wrote of the nights of "wonderful love" she shared with her husband, Charles, quite unashamed of "our love and of our desire" (p. 121). Elsewhere, Jalland notes that Lord and Lady Carrington "shared a bedroom," adding, without comment, "not the normal practice, by any means" (p. 96).

Both the joys and the dangers of bearing children emerge with acute poignancy in Jalland's study. Pregnant women did not take to the sofa as a matter of course as soon as their condition was discovered, but the number of miscarriages and the incidence of maternal mortality associated with miscarriages were so high as to provoke much anxiety about childbirth. Jalland is at her best and most compelling in relating the grief as well as the stoicism and courage of her elite subjects.

When she moves from the private to the political realm of these women, however, Jalland is less effective, in large part because the book lacks an organizing principle or even an argument by which to make the connections between the two spheres. We learn that political wives, sisters, and daughters had little influence on their husbands, brothers, or fathers beyond contributing to their peace of mind. After the passage of the Corrupt and Illegal Practices Act in 1883, elite women became far more prominent in campaigning and public speaking, which led many of them into the movement for women's suffrage. Jalland might have fruitfully explored the connections between public and private life that would help explicate this process, but the case studies she uses to examine political experiences are simply too short and disparate to sustain analysis. In fact the apparently disparate nature of the topics covered in the study acts to undermine the book as a whole. Jalland provides us with a great deal of important information, but we do not quite know what to do with it. The discussion of political wives follows sections on courtship and marriage and on childbirth and precedes a moving account of spinsterhood, without any framework for understanding how they might relate to one another. This is not to argue that the private and political spheres of Victorian and Edwardian women should be kept separate by historians; rather, they must be analyzed together in a coherent way so as to give meaning to each.

SUSAN KINGSLEY KENT
University of Florida

STANDISH MEACHAM. *Toynbee Hall and Social Reform, 1880–1914: The Search for Community*. New Haven: Yale University Press. 1987. Pp. xiii, 211. \$22.50.

This monograph contrasts sharply with *Toynbee Hall: The First Hundred Years* by Asa Briggs and Anne Macartney (1984). Whereas their book is a celebratory, comprehensive history, Standish Meacham criticizes that pioneering settlement house for failing to overcome class barriers. Canon Samuel Barnett founded the settlement in 1884 in the belief that poverty could not be ended by impersonal charities or welfare bureaucracies, that educated men had to reach out and serve the poor “one by one.” But after twenty years Barnett admitted that Toynbee Hall had failed to connect on that individual level. The likes of William Beveridge were using the institution as a sociological laboratory to investigate poverty and design state-run social programs.

The fault, Meacham concludes, was with the Toynbee residents. Although they professed to be disinterested mediators between the classes, they were in fact too “authoritarian” and paternalistic to establish real neighborly relations with the Whitechapel community. In their ivy-covered residence—Henrietta Barnett unabashedly said it was “exactly like a West-End drawing room, erring, if at all, on the side of gorgeousness” (p. 48)—Toynbee men lived a life apart from the East End.

This indictment is partly true but not entirely fair. The Toynbeeites stand accused of foisting middle-class cultural hegemony on the working class. They certainly were guilty of sponsoring popular lectures on the Tudor monarchs and eighteenth-century music. But Barnett insisted that his residents had “to learn as much as to teach,” and they learned volumes about slum life. According to Meacham, Barnett was “unwilling to acknowledge the extent to which the disinterested men . . . at Toynbee Hall were themselves representative of a class, or the degree to which the culture he hoped to broadcast was the culture of a particular elite” (p. 41). Actually, Barnett denounced university culture as hopelessly patrician, and he brought undergraduates to Whitechapel to jolt them out of “the narrow views which kept them as a class apart” (p. 38).

Of course, as Oxbridge men, the Toynbeeites were incapable of completely overcoming their elitism. What is remarkable is how far they succeeded. When Meacham says that Barnett had little sympathy with working-class culture, he is correct only if we define that culture to exclude trade unions, the cooperative movement, friendly societies, workers’ clubs, the 1889 dockers’ strike, and the Labour party: Toynbee residents were actively involved in all of them. Meacham is right

to conclude that the Workers’ Educational Association (WEA) came closer to Barnett’s ideal of personal fellowship across class lines, but he underplays Toynbee Hall’s crucial role in launching the WEA.

Perhaps the success of Toynbee Hall could only be judged by the East Enders themselves, but they hardly ever speak in this book. Meacham quotes some bitter comments by George Lansbury, who had personal grievances against Barnett. He does not quote the praises offered by Frederick Rogers, Thomas Okey, J. M. Dent, Tom Mann, and Ben Tillett. Meacham may depict the Toynbeeites as elitists, but Tillett found “no high-brow condescension” among them.

In Meacham’s eyes, the fact that so many Toynbee residents went on to become influential civil servants and leader writers suggests that they failed to establish close bonds of comradeship with the poor. Barnett did not think so: he wanted men who had worked in the slums to direct social policy—and so they did. Meacham discusses in depth the work of Beveridge and R. H. Tawney, but there were other influential Toynbee old boys, and we need to know more about their collective impact on British politics. Finally, Meacham does not attempt to write a general history of the London settlement houses: that is the focus of a forthcoming book by Seth Koven.

JONATHAN ROSE
Drew University

A. M. MCBRIAR. *An Edwardian Mixed Doubles: The Bosanquets Versus the Webbs: A Study in British Social Policy, 1890–1929*. New York: Clarendon Press of Oxford University Press. 1987. Pp. x, 407. \$72.00.

A. M. McBriar, whose *Fabian Socialism and English Politics* (1962) revised historical perceptions of Edwardian political thought, has written again of the same period and on some of the same themes in his latest book. He has chosen to focus on what he sees as the fundamental conflict between the ideas and proposals of two husband-and-wife teams of social reformers: Bernard and Helen Dendy Bosanquet and Sidney and Beatrice Potter Webb.

Initially, the scheme strikes the reader as an attractive one, not only because of the seeming contrast between the two couples but also because that conflict appears to epitomize the change from nineteenth- to twentieth-century ways of viewing the world and addressing its problems: the idealist Bosanquets, the positivist Webbs; the individualist Bosanquets, the collectivist Webbs; the Bosanquets, who argued that the key to social betterment was the reform of character, the Webbs, who

argued that the key was environmental improvement.

Yet, before reading very far, one begins to realize that perhaps these two "teams" were not as consistently on opposite sides of the net as McBriar's title and thesis would suggest. He himself is eventually forced to acknowledge that both couples were anxious to discover a way to reestablish community and that to this degree the Bosanquets could themselves be labeled collectivists. Both pairs, McBriar argues, sought a new "sociology" and a new "social economics" as the means of achieving social cohesion. "But within those general resemblances," he maintains, "lurked the differences between them of which they were so vividly aware" (p. 372). The more one considers the quartet, however, the more tempting it becomes to declare instead that behind an array of differences there lurked a surprising number of similarities.

Neither the Bosanquets nor the Webbs, for example, thought very highly of what Beatrice liked to refer to as "the average sensual man." Both believed that left to themselves the citizens in a democracy would choose the path of least resistance that led to debilitating dependence. Neither couple was much enamored of democracy itself. McBriar never makes clear the extent to which the Bosanquets and Webbs were committed to representative democracy. If, as he maintains, they gave it their blessing, it was a weak one. The Webbs criticized the Majority Report of the Poor Law Commission as undemocratic because it assigned the maintenance of the poor to Public Assistance Authorities unresponsive to the pressures of the electorate. Yet the Webbs were themselves attacked from the Left for their unwillingness to recognize that county council committees might be similarly aloof from the voting public. In fact the Bosanquets and the Webbs, together members of a late Victorian governing class with little faith in democracy, believed that experts must shape the policies that governments, in turn, must adopt in the best interests of the national community.

Tempting as it is, however, to point out these similarities, they cannot obscure one fundamental and all-important difference between the Bosanquets and the Webbs, one that McBriar, despite his extensive and extremely illuminating discussion of the Poor Law Commission of 1905-09, fails to emphasize sufficiently. The Webbs' demand for the establishment of a national minimum standard of living and their insistence that services be provided not by a destitution authority but by a variety of social services available to all not only distinguished their recommendations from the majority but established them as progenitors of a

new philosophy of social provision and of the twentieth-century welfare state.

STANDISH MEACHAM
University of Texas

DAVID BROOKS, editor. *The Destruction of Lord Rosebery: From the Diary of Sir Edward Hamilton, 1894-1895*. Atlantic Highlands, N.J.: Humanities or Historians. London. 1986. Pp. x, 290. \$50.00.

Lord Rosebery became leader of the Liberal party and prime minister when W. E. Gladstone resigned in protest against increasing naval expenditures in March 1894. His was a difficult legacy. As a peer, Rosebery had to leave business in the House of Commons in the hands of Sir William Harcourt, a capable but fractious politician. The Liberal party was badly fragmented and dependent for its small majority on the votes of Irish Nationalist M.P.'s. Politically, Rosebery was relatively inexperienced at the cabinet level and had neither a comprehensive vision nor the determination to force through a legislative program. Personally, he still grieved over his wife's death in 1890, hesitated to display strong ambition, and was drained by insomnia. Perhaps he was fortunate that his premiership lasted only sixteen months before Lord Salisbury replaced him, and the Unionists in July 1895 gained the largest Conservative electoral victory since the Reform Act of 1832.

Despite its dramatic title, this book presents a political history of Rosebery's administration. About 40 percent of the text is a narrative by David Brooks that traces the events noted in Sir Edward Hamilton's diaries between February 1894 and August 1895. Excerpts from those diaries, which are deposited in the British Library, make up the other 60 percent of this volume. Although the story of Rosebery's downfall has often been told, notably by Peter Stansky in *Ambitions and Strategies* (1964), Brook's account provides a useful introduction to the main attraction of the book, Hamilton's diary entries.

Hamilton was in the mid-1890s assistant financial secretary to the Treasury, but he was no ordinary civil servant. The eldest son of a bishop of Salisbury, he had the social advantages of an education at Eton and Christ Church. At both places he was a classmate of Archibald Philip Primrose, who in 1868 became the fifth earl of Rosebery. After leaving Oxford, Hamilton began a career at the Treasury. In 1872 the chancellor of the Exchequer, Robert Lowe, chose him as one of his private secretaries. He was well suited to this role by his abilities and personal qualities, particularly his tact and patience. He served Gladstone

as private secretary during the Liberal government of 1880–85, the highlight of his career. Although he afterward returned to the Treasury, Hamilton, a lifelong bachelor, remained on close terms with many of the leading Liberal politicians. From 1880 until 1906 he recorded his observations of politics above all, but also of society, culture, and sport in a series of diaries. Dudley W. R. Bahlman has published the most significant years complete in *The Diary of Sir Edward Walter Hamilton, 1880–1885* (2 vols., 1972), with an excellent introduction and informative footnotes.

This publication of Hamilton's diary for 1894–95 is not up to the high standard set by Bahlman and the Clarendon Press. Copyediting seems to have been lax, identifying notes are placed at the end, and the index is incomplete and inconsistent. Although the excerpts focus on politics, Brooks has fortunately left in Hamilton's remarks on such diverse subjects as the trial of Oscar Wilde, the batting feats of the cricketer W. G. Grace, and the attempt to rid the Empire Theatre of prostitutes. The keen and lucid commentary of Hamilton remains a pleasure to read as well as a source of rare insight into his life and times.

MARVIN SWARTZ
University of Massachusetts

LORD BLAKE and HUGH CECIL, editors. *Salisbury: The Man and His Policies*. New York: St. Martin's. 1987. Pp. ix, 298. \$29.95.

It is surprising how little attention has been paid to the third marquis of Salisbury; the four-volume biography by his daughter, Lady Gwendolen Cecil, published between 1921 and 1932, still ranks as standard. And yet it was Salisbury who presided over the transformation of the Conservative party into a party of government—arguably into the kind of party it remained until the advent of Margaret Thatcher. The collection of essays under review here, relying heavily on Salisbury's papers held at Hatfield House, begins to make amends for past neglect.

The nine substantive essays fall into three categories, each with a different readership in mind. The first group, the biographical, includes J. F. A. Mason's "Lord Salisbury: A Librarian's View" and Lady Gwendolen Cecil's "Lord Salisbury in Private Life" (originally written between 1906 and 1911, printed for limited circulation in 1949, and now made available to the public for the first time), and, accompanying them, Hugh Cecil's "Lady Gwendolen Cecil: Salisbury's Biographer." Taken together, these essays—along with Lord Blake's introduction, an elegant sketch of the protago-

nist—evoke a "profound pessimist" who "believed that the state of politics was getting worse," but who nonetheless determined to do "all he could to slow down the process" (p. 8).

Whereas the first group of essays can be enjoyed by the general reader, the second group is aimed at the specialist. The two by E. D. Steele, "Salisbury at the India Office" and "Salisbury and the Church," as well as Robert Stewart's "'The Conservative Reaction': Lord Robert Cecil and Party Politics," belong in this category. In each of his contributions Steele seeks to fill acknowledged gaps, and he provides a wealth of new detail; each, however, is wretchedly written. Stewart's essay covers more familiar ground: Salisbury when he was still Lord Robert Cecil and becoming famous, or infamous, for his outspoken condemnation of Benjamin Disraeli. Stewart is thorough, so too is Steele, and all three contributions are long on trees and short on forests.

The third group of essays should appeal to broad-gauged historians. The common theme that unites them and enables the reader both to appreciate Salisbury's individuality and to view him as a representative figure might be summed up in one word, constraint—how Salisbury worked within clearly recognized limits and adapted to realities not of his choosing. F. M. L. Thompson concentrates on the private purse. In his essay, "Private Property and Public Policy," he examines how Salisbury husbanded his resources. He paints a picture of a paternalist rural landowner "acknowledging moral obligations and social responsibilities for his tenantry, servants and dependents," while businesslike in his philanthropy, and of an urban landlord "interested in maximising financial returns . . . from his property, without countenancing shady practices or jerry builders" (pp. 281–82). A. N. Porter focuses on the public purse. In his contribution entitled "Lord Salisbury, Foreign Policy and Domestic Finance," he stresses how constraints engendered by the taxation system fitted in with Salisbury's preferred style of diplomacy, how his wish to hold down direct taxation required "a kind of diplomacy . . . which would not only avoid crises and the expenditure attendant upon them, but also keep the general level of expenditure to a minimum" (p. 158). And, finally, John France, in a first-rate essay, "Salisbury and the Unionist Alliance," concentrates on politics. The framework of political action, he argues, is "best understood in terms of degrees of Liberalism. Policy options were seen as appearing on a line between inaction, at one extreme, through Liberalism and Radicalism, leading on eventually to Socialism at the other" (p. 234). France takes pains to show how Salisbury situated himself on this continuum. As a young man, he

had unabashedly espoused the interests of his own class; he had always, however, charted a narrow field for personal action. And that very modesty, coupled with his pessimism, eventually led him to define a conservative position that did not fundamentally challenge the liberal society Britain had become.

JUDITH M. HUGHES
*University of California,
 San Diego*

BENTLEY BRINKERHOFF GILBERT. *David Lloyd George: A Political Life; The Architect of Change, 1863–1912*. Columbus: Ohio State University Press. 1987. Pp. 546. \$40.00.

In 1963 Kenneth Morgan expressed his fear that David Lloyd George might always remain an enigma. Ten years later he was hopeful that a more complete and balanced portrait of this political genius was slowly emerging. Bentley Brinkerhoff Gilbert has now contributed substantially to our understanding with this first volume of his full-scale scholarly biography of Lloyd George as the modern "archetypal political man" (p. 9).

Gilbert concentrates on "high politics," dividing the seven massive chapters almost equally between Lloyd George's career as Liberal backbencher to 1906 and his early years in office to 1912. Gilbert skillfully charts the story of "near disasters and revivals, of clinging too long to hopeless campaigns," followed by triumphant returns to popular favor (p. 158). Lloyd George emerges as a political genius who was not always an attractive man—a great salesman who instinctively sold himself before the product.

Although much of this story is familiar, the wealth of detail is often new, and the interpretation is sometimes unexpected. Gilbert discovers more consistency and continuity than might be anticipated. Lloyd George had a remarkable talent "for improvising policies towards an unknown goal" (to quote A. J. P. Taylor). Thus, the foundations of the famous land reform campaign of 1912 can be traced to the early 1890s, when Welsh rural grievances formed his first rallying cries. Indeed, land reform was "the great unifying thread of his political life" (p. 142). After 1895 Lloyd George simply applied his Welsh reform proposals to the rest of Great Britain.

Gilbert's more unusual argument is that Lloyd George's social reform program of 1908–12 was rooted in his instinctive political response to Joseph Chamberlain's tariff reform scheme. Lloyd George recognized immediately that tariff reform could revitalize the Conservative party. Indeed,

after 1906 a revived, protectionist Unionism emerged, "demagogic, violent and reckless," prepared to win working-class votes with promises of cheap social welfare financed by import duties (pp. 267, 362). Lloyd George assessed Chamberlain's "demagogic Unionism" as Liberalism's chief competitor for working-class votes and replied to the challenge with the equally ambitious and constructive program of New Liberalism. (Gilbert contends that the origins of Liberal social reform "have nothing to do with the Labour Party as such" [p. 277]), although in his 1966 book he argued that national insurance was the Liberal response to the threat of socialism.)

A few qualifications are necessary. Gilbert could have usefully located his own major arguments more precisely in the context of the different views of earlier writers. He does not explain fully enough why none of the recent full-scale biographies "is satisfactory . . . as a work of scholarship." Further, Gilbert is interested in immediate political developments somewhat at the expense of the broader social and intellectual context. For example New Liberalism is treated occasionally as if it were chiefly "a maneuverable tactical program" (p. 142). But the "revolution in public attitudes" (p. 336) helps explain the political climate in which Lloyd George operated and deserves further analysis.

Gilbert's decision to exclude details of Lloyd George's personal life, on the grounds that "for his political career they are extraneous" (p. 61), must be challenged. He argues that for Lloyd George "women in general [were] for relaxation" (p. 61); they were "only a diversion" to be compared with Herbert Asquith's bridge or Franklin D. Roosevelt's stamp collection (p. 9). Lloyd George's will to succeed "precluded any lasting attachment to women" (p. 61). But the author equates Lloyd George's two long-term and significant relationships with the women he married with his casual affairs and flirtations, which were indeed unimportant. Kenneth Morgan and John Grigg have shown that Lloyd George had an enduring affection for his first wife, Margaret, and that the peaceful domestic base in Wales provided the essential security and reassurance he needed for his early political progress. It is almost as odd for Gilbert to try to exclude Lloyd George's personal life from this biography as for John Morley to seek to ignore the role of religion in his biography of William Gladstone.

Gilbert has succeeded admirably in his prescribed task. This volume is based on exhaustive research and makes an important scholarly contribution to our understanding of Lloyd George's early political career to 1912.

PAT JALLAND
Murdoch University

PETER DENNIS. *The Territorial Army, 1906–1940*. (Royal Historical Society Studies in History Series, number 51.) Wolfeboro, N.H.: Boydell and Brewer, for the Society, London. 1987. Pp. 274. £25.00.

The growing interest in what might be termed Britain's "amateur military tradition"—auxiliary forces such as the militia, yeomanry, and volunteers—has ensured that many of the gaps in our knowledge have been filled in recent years. This is all the more commendable since, in many cases, few records have survived in national archives. Would-be historians of the auxiliaries have perforce to undertake lengthy tours of local archive repositories. Peter Dennis is the latest to do so, and his account of the Territorial Army between 1907 and 1940 bears witness to his endeavors in well over thirty county record offices.

Some of the narrative covers familiar ground, notably the chapters on the pre-1914 period, while Dennis's excellent analysis of the role of Territorials in aid of the civil power has already appeared in the *Journal of Contemporary History* (1981) and his thoughts on post-1918 reconstitution in the collection of essays published as *Swords and Covenants* (1976). He is also unfortunate in the length of time it has taken for his book to be published since he has been unable to refer to recently published work on the Territorial role in World War I. Nevertheless, his book is still welcome as a comprehensive survey of the difficulties experienced by the Territorials between the wars.

For all the local character of the Territorials, their fortunes reflected the wider context of national defense policy, and Dennis is a sure guide to the implications of that policy. He has previously published a study of the debate on conscription in interwar Britain, and the present study is a logical extension. Certainly, a major problem for the Territorials after 1919 was their precise military role when they were unsuited either for a garrison role in the newly expanded empire or for unlimited service in medium-scale conflicts falling short of those for which conscription would be reintroduced. What made the Territorials' employment especially problematic was their wariness of undertaking any wider obligations without firm guarantees of the integrity of their units. The so-called pledge was the price of Territorial agreement to a general liability for overseas service, but one still so qualified that it bedeviled the relationship between the War Office and the Territorials for twenty years. The struggle over the pledge rightly forms the core of Dennis's narrative, but, since so many difficulties were a direct legacy of World War I, it is surprising that he actually devotes so little attention to the war itself.

Indeed, much more should have been made of wartime experiences both between 1914 and 1918 and between 1939 and 1940, and Dennis consistently underplays the considerable tensions that continued to exist between the regular army and the Territorials. Further, Dennis has not quite glimpsed some of the continuities of the amateur military tradition that render the Territorial experience far from unique, but it is a measure of his achievement that his contribution brings us much nearer to being able to produce an overall synthesis of that tradition.

IAN BECKETT
Royal Military Academy

TIM TRAVERS. *The Killing Ground: The British Army, the Western Front, and the Emergence of Modern Warfare, 1900–1918*. Boston: Allen and Unwin. 1987. Pp. xxiv, 309. \$34.95.

Tim Travers has written an important book on a subject usually neglected by the professional historian: the ideas, tactics, and principles employed by the British Expeditionary Force (BEF) in the siege warfare of the western front from 1914 to 1918. The recent literature concerning war and society and the higher conduct of the war has been impressive for both its quality and quantity; with few exceptions, however, recent interpretations of the bloody business of fighting the German army have been left to popular historians and professional soldiers, who have, moreover, concentrated far more on the common soldier or command decisions than on the evolution of tactics.

Travers has searched the abundant store of primary sources now available, though frequently neglected by popularizers of battlefield history, especially the War Office files and private papers and diaries of officers. He has also employed some of the techniques—and occasionally the language ("group think" and "paradigms," for example)—of the social scientist, emphasized the history of ideas, and begun his study of British army wartime leadership in the prewar years. The result is an account that is truly enlightening and unlike any previous examination of his subject, with the possible exception of *Fire-Power: British Army Weapons and Theories of War, 1904–1945* (1982) by Shelford Bidwell and Dominick Graham.

Part 1 focuses on the Edwardian senior officers corps. Chapter titles such as "The System at Work: Promotions, Dismissals, and the Personalized Army" and "The Cult of the Offensive and the Psychological Battlefield" reflect Travers's novel approach. Part 2 is a critical evaluation of the ideas and personality of Sir Douglas Haig and is the most revealing examination of the generalship of

the enigmatic commander in chief of the BEF yet written. Part 3 treats the controversial Somme campaign in light of the interpretation presented in previous chapters. The last section, part 4, critically evaluates the Official History's treatment of the Somme and Passchendaele offensives. The epilogue, a comparison of the leadership of the British, French, and German armies, is somewhat disappointing. This subject is worth another monograph instead of the twelve pages of analysis provided.

Travers concludes that many senior officers failed to adapt to the new technical warfare. Their nineteenth-century image of warfare led them to believe that the soldier behind the weapon was more important than the weapon. Thus, senior officers tried but usually failed to come to "mental grips with the tactical and command changes implied by the new or improved technology" (p. 253). Haig emerges as an aloof and isolated commander clinging "to an earlier paradigm of war" (p. 252) that he had learned at Staff College in 1896–97. Poor communications between Haig, General Headquarters, and the army commanders are shown to have created a command vacuum that hampered the execution of the Somme and Passchendaele offensives and the initial response to the German offensive in 1918. Some of Travers's conclusions are certain to be debated, but they cannot be ignored.

This fine book is only marred by the author's occasional acceptance as evidence of what amounts to hearsay or gossip in the letters and diaries of senior officers. As one example, Travers, citing Sir Henry Wilson's diary, asserts that Sir William Robertson, the chief of the Imperial General Staff, considered sacking Haig and taking his place during the disastrous opening stage of the Somme offensive. This assertion is completely at odds with the historical record. Robertson without question disagreed with Haig's conduct of the offensive, but I know of no firm evidence that he actively sought—or even privately desired—Haig's replacement in July 1916, especially by himself.

No graduate or undergraduate library should be without this valuable and provocative study.

DAVID R. WOODWARD
Marshall University

WAYNE LEWCHUK. *American Technology and the British Vehicle Industry*. New York: Cambridge University Press. 1987. Pp. viii, 304. \$44.50.

Henry Ford revolutionized factory production through his use of the moving assembly line and other techniques at his Highland Park, Michigan, plant during the 1910s. Ford imposed a new

"effort bargain" on his employees through deskilling of the work force, reliance on mechanical pacesetters, the expansion of managerial power, and a dramatic increase in wages through the five-dollar-day. British vehicle manufacturers did not adopt Fordist mass production techniques but instead accepted low output levels, poor worker productivity, and relatively high product prices until the late 1970s. American and British observers alike have variously blamed short-sighted capitalists, obstinate workers, or the small domestic market for this failure. Wayne Lewchuk, however, argues that Britain's automobile industry developed a unique and successful version of mass production that was better suited to British conditions.

The author is an economic historian who employs economic analysis, game theory, and traditional historical analysis in this study. He argues that technological change must be understood as one of several methods capitalists employ to alter the "labor effort bargain," that is, the trade-off between work effort and earnings. Both capital and labor can attempt to shift the labor effort bargain in their favor through institutional changes, such as innovative wage payment systems or labor unions. He uses game theory to help explain the failure of British manufacturers to adopt Ford's methods.

British industry in the early twentieth century, including the first generation of automobile firms, adopted mechanized production, but capitalists were not able to gain control over effort levels. Strong trade unions and worker mistrust of capital combined to keep industrial management weak and undeveloped. As a result, British industry settled on a low wage–low effort compromise.

The Ford Motor Company successfully transferred its Detroit production methods to its Manchester works in 1914. The Associated Equipment Company, a bus manufacturer, adopted Fordist assembly line methods in 1917, but the rest of the vehicle industry did not. Instead, capital used a variety of incentive pay schemes to control production and effort levels. Labor, rather than management, coordinated shop-floor production, compensating for the lack of management skills.

The author argues that these methods, which he calls the "British system of mass production," worked well during the interwar period, when high unemployment levels and weak labor unions were the rule. During the 1950s, however, when full employment was the norm, labor unions exploited this system, shifting the effort bargain sharply in favor of labor. Productivity shrank, profits disappeared, and the motor vehicle industry collapsed.

Lewchuk's characterization of the British motor

vehicle industry during the interwar period as a "success" is hard to swallow. Compared to their American and Continental counterparts, British vehicle manufacturers were small, inefficient, technologically backward, and unprofitable. If this industry is a microcosm of the British economy in the twentieth century, Lewchuk's work contains important insights into the dynamics of industrial decline. This is an innovative and provocative analysis of technological change and a valuable addition to the literature.

CHARLES K. HYDE
Wayne State University

JIM FYRTH. *The Signal Was Spain: The Spanish Aid Movement in Britain, 1936–39*. New York: St. Martin's or Lawrence and Wishart, London. 1986. Pp. 344. \$32.50.

This study of the movement to aid the Spanish Republic during the Civil War begins as an account of the various medical groups, staffed by British volunteers and supported by British contributions, that were sent to assist in the care of the wounded among the Republican forces. Jim Fyrth charges that historians since World War II have deliberately neglected this story because the cold war made such manifestations of leftist solidarity retrospectively suspect. In fact the effort, although undoubtedly relieving much pain and suffering, was relatively limited, and Fyrth's careful chronicle of the comings and goings of this handful of medical personnel is of little more than antiquarian interest.

If the impact of the various medical aid groups on the situation in Spain was slight, the strenuous efforts of those in Britain who supported various forms of assistance to the Republicans ranging from the shipment of food through the provision of care for four thousand Basque refugee children to the campaign against the British government's policy of nonintervention constitute a substantial grass-roots movement on the Left, and it is in its examination of these efforts that this work gains significance. Drawing on the minutes of myriad committees, a copious pamphlet literature, memoirs, and interviews, Fyrth describes an energetic movement that mobilized workers and intellectuals alike. Although the appeals were organized by activists, many of them Communists, the author argues persuasively that the promptness and generosity of the response from all sections of the working class, even in communities afflicted with widespread unemployment, indicate that the issues at stake in Spain were of deep concern to British workers.

Paul Johnson has observed that no event in the

twentieth century has been more lied about than the Spanish Civil War. Fyrth does not so much lie as repeat the half-truths and distorted emphases propagated, and possibly believed, by the extreme Left in the 1930s almost as if decades of scholarship had not painted a far more complex picture of the Spanish tragedy. He sees the Republicans as involved in an uncomplicated struggle against fascism and in defense of democracy. The murderous hatreds among the various Republican factions are dismissed briefly. The savage suppression of the *Partido Obrero de Unificación Marxista* (POUM) by the Communists in 1937 is described thus: "The P.O.U.M. militia units were disbanded, some of its leaders imprisoned and some shot" (p. 85). This laconic account scarcely describes the torture and execution of thousands upon thousands of leftists, some of them recalled from the front for the purpose. Likewise, the picture presented of the British political scene in the 1930s is far from accurate. Fyrth not only holds the National Government responsible for the appeasement of the dictators, which of course it was, but implies that the British Left invariably opposed such policies, which, for the most part, it did not.

Although these important questions are treated in ways that are far from satisfactory, the focus of Fyrth's study, the account of the movement to aid Republican Spain, contains much of value to the historian of the British Left. The appeal of the cause, as he demonstrates, energized many hitherto uninvolved in politics and provided them with a political education that prepared them to participate actively in the politics of the postwar decades.

CATHERINE ANN CLINE
Catholic University of America

PAUL BEW. *Conflict and Conciliation in Ireland, 1890–1910: Parnellites and Radical Agrarians*. New York: Clarendon Press of Oxford University Press. 1987. Pp. 241. \$47.00.

Paul Bew has made a valued contribution to what has become a formidable list of revisionist studies in Irish historiography. This work focuses on the diverse agrarian problems that confronted the Irish Parliamentary party (IPP) between the fall of Charles Stewart Parnell and the Third Home Rule Bill crisis and also on the quality of John Redmond's leadership in addressing the challenge posed by those problems.

Throughout the book Bew offers reflective judgments about the complex relationships between agrarian classes, including the rural bourgeoisie, poor peasantry, and agricultural proletariat, and further develops a few of the themes that

he first advanced in an earlier study, *Land and the National Question in Ireland* (1978). To wit, the author effectively illustrates how profound were the social and political divisions within rural Irish society and how ill-suited Redmond was for the task of reconciling those divergent forces on behalf of the nationalist movement.

Redmond shared Parnell's conciliatory policy toward landlords in the belief that once the land question was resolved in a fashion that was agreeable to landlords and tenants alike a substantial number of landlords would embrace Home Rule and thereby strengthen the credibility of that cause at Westminster. Redmond also had Parnell's habit of failing to appreciate that Ireland contained not one but two nations, and both men tended to underestimate the threat of Ulster Unionism. But, while Parnell could be charismatic and forceful, Redmond was neither. His often indecisive style, which was seen by some as political timidity, may have been at least partly attributable to the sometimes contradictory stances that Redmond was forced to adopt, depending on whether he was in Ireland or at Westminster.

Although Bew might be faulted for giving too much attention to the inconsistencies within the United Irish League (UIL) and too little to its successes, he makes the telling point that the UIL was not a new version of the Land League, which, after all, had had different objectives and methods. The author also reminds us that UIL founder William O'Brien supported Redmond for the leadership of a reunited IPP in 1900 in the belief that he could be manipulated. But if that strategy did not succeed it was not because Redmond was resolute in his own course of action. He was equivocal about the merits of the Wyndham Land Purchase Act of 1903, particularly as it affected the west, and he reversed himself entirely a few years later on the policy of conciliation with the Irish Unionist minority. Nowhere, however, was the failure of leadership more revealing than in Redmond's tentative response to the outbreak of agrarian violence in 1907 over the issue of cattle driving. In condemning the methods of such radicals but not necessarily their objectives, Redmond offered no clear direction of his own and thereby contributed to the weakening of constitutional nationalism at a time when advocates of physical force were becoming ever more assertive.

Bew demonstrates a good critical grasp of the basic archival sources and of the recently published literature. He has, for example, made extensive use of the pertinent police and official reports, together with the relevant political correspondence, and he has consulted the now available papers of Michael Davitt and John Dillon. Bew has also drawn on more than eighty newspapers, the

bulk of which represent rural regions, and they constitute an invaluable resource that he employs to good advantage.

If this otherwise splendid book has one conspicuous flaw, it is one for which author and publisher have a shared responsibility. A proper name that appears in the references is spelled differently on pages 119 and 135, Dennis Johnston becomes Dennis Johnstone on page 141, there are two footnotes 202 in the text on page 165, and so on. But these and other misprints are more distracting than serious. Errors of fact are rare, as in the instance of page 150 where the February 1908 date of C. J. Dolan's by-election is given as April 1908. Such quibbles aside, however, Oxford University Press is to be commended for placing references at the bottom of the page. And Bew must be credited for providing us with an authoritative account of the agrarian factors and factions relating to Irish constitutional nationalism between 1890 and 1910 that helps us better understand why the Irish Parliamentary party responded as it did to critical challenges, such as the 1906–08 Ranch War, and to more lasting problems, such as the Unionist tradition.

THOMAS E. HACHEY
Marquette University

LAWRENCE M. BRYANT. *The King and the City in the Parisian Royal Entry Ceremony: Politics, Ritual, and Art in the Renaissance*. (Travaux d'Humanisme et Renaissance, number 216.) Geneva: Droz. 1986. Pp. 310.

Lawrence M. Bryant's scholarly study of Parisian royal entries over a two-hundred-fifty-year period fills a void in the growing work of historians interested in political culture. Bryant has collected the many manuscripts and printed documents that recorded these events in order to reconstruct them (the text includes forty-nine plates and eleven analytical tables), and his reconstruction sheds light on how the performance of royal entries both articulated and reflected concepts about the early modern state. This new interpretation views the ritual of the entry in dynamic terms, placing it within the context of the society that engendered it, rather than in static terms, viewing it as the mere product of Renaissance culture. The royal entry into Paris first produced a civic drama that crossed the grid of status (encouraging representation of varied orders) and then later changed form and produced a singularly royal performance (stressing dynastic and aristocratic attributes). From the late fourteenth through the late seventeenth centuries, the ritual addressed a national, even international, audi-

ence, it articulated traditional constitutional precepts, and it finally redefined relations between the ruler and the ruled to usher in a new era. By connecting critical constitutional issues with shifts in entry programs, Bryant shows how the guarantor of civic liberties shifted in the sixteenth century from juridical and corporate bodies (guilds, Parlement of Paris) to individual and dynastic heroes of the seventeenth century, signaling the advent of absolutism.

According to Bryant's analysis, the greatest age of the French royal entry was 1484–1571 when the ceremony was participatory—encompassing a variety of persons and institutions essential to governance. At that time the entry ceremony joined dynamic and static theories of state: first by dramatizing constitutional precepts in a public theater; then by reinforcing such precepts through the public distribution of treatises and engravings (exemplified in the plates) that recorded the performance. By contrast the decline of the royal entry dates from Louis XIV's entry of 1660, which both revived and shattered the traditional program. Rather than stressing the constitutional themes emphasizing the union of corporate authorities in governance, later ritual glorified the king's august person. Following this formidable shift of venue, the curtain was drawn on the public stage of the Paris streets, and different royal ceremonies were adopted in the confines of the court.

The themes treated in this study give some sense of the richness of this monograph for students of political culture: the relation of the Renaissance entry to the earlier *Droit de Joyeux Avènement à la Couronne*; the analysis of the entry's Parisian participants; the ritual role enacted by kings and queens; the symbolic significance of pageantry, mystery plays, emblems, and so on, displayed at the Saint Denis Gate, the Fountain of Ponceau, the Hospital of the Trinity, the Painters' Gate, the rue Saint Denis, and the Chatelet; and accounts of the last of the entry ceremonies at the bridges and the Palais de Justice. Over time, it is clear, changing notions of authority and community were recorded in the programs for royal entries. During the earlier period the French royal entry propagated a constitutional image of the kingdom, signifying reciprocity between king and subjects, but during the later period the ritual propagated a more absolutist image of royal governance, emphasizing, for example, the right of the king to tax subjects. Years ago Ernst H. Kantorowicz presented evidence from eminent English and Continental legists to show how the metaphor of "the king's two bodies" offered a conceptual distinction between the king (as an individual) and the King (as an officeholder), the

separation of the person (who will die) and the office (which never dies). Now, in this formidable study, Bryant documents how the French actually acted out, or expressed, this constitutional concept through the ceremony of the royal entry during the fifteenth and sixteenth centuries before its meaning was lost to human memory.

SARAH HANLEY
University of Iowa

ROGER ZUBER and LAURENT THEIS, editors. *La révocation de l'Edit de Nantes et le protestantisme français en 1685*. (Société de l'histoire du protestantisme français. Actes du colloque, 1985.) Paris: The Society, with the cooperation of the Centre National des Lettres and the Centre National de la Recherche Scientifique. 1986. Pp. 392. 180 fr.

Editors Roger Zuber and Laurent Theis present a collection of nineteen papers delivered at a conference held in Paris in October 1985 to mark the tercentenary of the revocation of the Edict of Nantes. The essays are divided into three sections—the actors, the victims, and the witnesses. (In terms of subjects, the obvious omissions are the Huguenot exodus and the refugee communities, but other books published in 1985 and 1986 have focused on those topics.) In an ecumenical spirit, the authors find fault almost everywhere, with Catholics (especially the clergy), Huguenots (who ascribed to the same brand of Augustinian intolerance as did their adversaries), Jean-Baptiste Colbert, Michel Le Tellier, the marquis de Louvois, various intendants, and, of course, Louis XIV. While Pierre Chaunu argues that the Sun King's thinking was cloudy in 1685 owing to illness, Jean Carbonnier emphasizes that the inclusion of the phrase "perpetual and irrevocable" in the Edict of Fontainebleau was intended to make clear that the revocation had not been issued capriciously. Chaunu is on firmer ground in stressing that toleration was not an acceptable program in the seventeenth century, that both Catholic and Protestant Europe desired unity of religion, and that the revocation echoed the Edict of Nantes in opposing toleration. Jean Delumeau likewise takes this position as he traces the development of the idea of toleration from the sixteenth to the eighteenth century. The Edict of Fontainebleau was, then, very much a part of the seventeenth-century mind; the treatment of Huguenots was comparable to the English penal laws against Catholics, to England's treatment of the Irish, and to Spain's behavior toward the Moriscos. In a comment on Jacques Le Brun's paper ("La conscience et la théologie moderne"), Elisabeth Labrousse illustrates clearly the era's disgust for toleration by

recalling that Antoine Arnauld, while vigorously protesting the persecution of Jansenists, did not write one word against the revocation. Carbonnier points out that the originality of the Edict of Fontainebleau lay in its denial to the Huguenots of the *jus emigrandi*, the right to emigrate, a provision accorded minorities in the Peace of Westphalia.

Individuals and groups, rather than grand themes, are the subjects of most of the papers. Pierre Bolle examines two bishops, Etienne Le Camus and Daniel de Cosnac. Both sought the conversion of Huguenots, the former by "charitable" means (preaching and teaching) and the latter sanctioning such violent methods as the demolition of temples and the use of *dragonnades*. Both bishops failed. Robert Sauzet's analysis of the bishops of Languedoc demonstrates failure there as well; the prelates simply could not understand the Huguenots' refusal to convert. Robert Poujol discusses four intendants in two provinces, Languedoc and Poitou. Politics and religion figured in the intendants' motives: they wanted to impress the king with a brilliant feat, and they desired to do God's work. It is interesting that the most mediocre intendants, René de Marillac and Nicolas Foucault, were most enthusiastic about the *dragonnades*. Henri Daguesseau returned to Paris rather than deal with *dragonnades* in Languedoc; he was one of the few to transcend the mentality of an era that justified violence in the pursuit of ends. Sauzet does not exonerate the king, who realized early on that the general conversions were a facade. Janine Garrisson describes the works of authors who influenced the writing of edicts and declarations against the Huguenots. She feels they took an unfair and narrow view of the Edict of Nantes, attacking its validity by claiming it had been extracted by force and identifying seventeenth-century Huguenots with their rebel ancestors. Garrisson is too one-sided in her opprobrium; the Catholic authors were correct in maintaining that the Huguenots had wrested the Edict of Nantes from Henry IV.

The essays on the "victims" make generalizations difficult indeed. Jean-Pierre Poussou and Philippe Loupès prove that the condition of Bordelais Huguenots was not as bad as that of their coreligionists elsewhere: there were no *dragonnades* and no mass exodus after the revocation. Textbooks might state otherwise, but Bernard Vogler shows that Alsace was not exempt entirely from the revocation. Resistance there did force that grand bully, Louvois, to back down. Jean-Paul Pittion discusses the Protestant academies from the Edict of Nantes to the revocation. Olivier Fatio the pastor Claude Pajon and the last great theological controversy among the Reformed churches of France, Elisabeth Labrousse the first "Lettres

pastorales" sent to Huguenots in France, and Solange Deyon the destruction of the Huguenot temples. A great psychological blow to the Huguenots and a sign of their defeat, the demolition of temples signaled victory for Catholics as well as revenge for the iconoclasm of sixteenth-century Huguenots. With a perversity worthy of religious bigots, the government had the Huguenots pay for the demolition; thus, they could choose to tear down their own places of worship in order to be spared the costs and to keep the buildings' stones.

Under the rubric "witnesses," Pierre Blet argues that Pope Innocent XI did not, as many historians have believed, condemn the revocation. Hans Bots examines the reporting by publications in the United Provinces of the treatment of Huguenots in France; Eric Roland Briggs mines eight letters written by a "new convert" to Catholicism; and Hélène Himelfarb paints the relationship between art, royal academies, and the revocation. In a fascinating piece, Philippe Joutard looks at the history of the commemorations of the revocation and makes the obvious but important observation that each commemoration says as much about the present as about the event commemorated. That in 1885 provides an enlightening comparison to the 1985 meeting in Paris. The bicentennial was ecumenical but intensely nationalistic—scarcely anyone attended from outside France. The tercentenary was international, with participants from twelve countries, including Germany, whose representation was no doubt made agreeable by the Franco-German rapprochement of this century.

Finally, Zuber summarizes unifying elements that came out of the conference: the importance of theology and the religious attitudes of persecutors and persecuted, the religious fervor of the Huguenots (thus disclaiming Emile Léonard's thesis of "le protestantisme en léthargie"), and the resistance of the Huguenots throughout the century.

The conference in Paris generally eschewed partisan polemics, whether Catholic or Protestant. We are, the authors implicitly tell us, sufficiently different from our seventeenth-century ancestors that we should feel no pain when their actions are called to account. This is a fine book, with essays of high quality.

RICHARD M. GOLDEN
Clemson University

HENRY LAURENS. *Les origines intellectuelles de l'expédition d'Égypte: L'Orientalisme islamisant en France (1698-1798)*. (Varia Turcica, number 5.) Istanbul: Isis. 1987. Pp. 258.

Historians of the Middle East are unanimous in regarding the French invasion of Egypt in 1798 as a watershed in the history of the region. The French expedition marks the first full-scale incursion of Western imperialism into the Arab heartland of the Ottoman empire, the first assault against Islamic self-complacency, the first impetus toward modernization and reform along Western lines—a process that would ultimately galvanize Egypt and its Arab neighbors into modern nation-states. There is less unanimity, however, on the motives that prompted France to embark on such a military adventure at this particular juncture in the revolution. Among the reasons most frequently advanced is that, taking the Directory's orders to General Bonaparte at face value, the principal objective of the invasion was to deliver a crippling blow against Britain's commercial supremacy by launching an attack against its overland communications with India. Other historians, stressing colonial and strategic motives, trace its origins to a renewed imperialist drive—the first step in a partition of the Ottoman empire designed to offset by the seizure of a Mediterranean colony the losses that France had suffered in Canada and the Caribbean.

It has also been argued that the expedition, especially when studied in terms of its more immediate origins, was to a large extent the result of a fortuitous set of circumstances, the unforeseen consequence of a combination of historical forces and personal calculations only remotely concerned with the ultimate fate of Egypt or Islam. According to this view, the opening chapter in Egypt's first encounter with the West did not have its origins in any deliberate policy designed to undermine Britain's economic might or extend French power overseas but was rather the accidental by-product of the struggle for power between a dynamic French revolution and the monarchies of the Old Regime. Such personal factors as Bonaparte's ambition, the Directors' overriding desire to rid themselves of an intriguing general, and the independent line pursued by Foreign Minister Talleyrand combined to deflect the revolution on an Eastern crusade. It was thus the tensions, both internal and external, generated by the French revolution itself that impelled a vacillating government to endorse a colonial venture originally envisaged by Talleyrand as the best way to curb his country's revolutionary zeal and restore the balance of power in Europe.

Readers already familiar with the relevant historiography on the French expedition will find that Henry Laurens's monograph has little to add to the political, diplomatic, and economic dimensions of this episode. The purpose of his study is at once more modest and more ambitious. It sets out

to show that from the larger perspective of intellectual history the invasion of Egypt was in fact the end result of a liberal current of eighteenth-century ideas aimed at eradicating the evils of Oriental despotism in the name of the French Enlightenment. "The Egyptian expedition," argues the author, "is far from being a historical accident resulting from political and strategic considerations. It is the testing ground for a new colonialist spirit designed not only to achieve economic expansion but, more broadly, to Westernize the rest of the world" (p. 192). Such an argument has the curious effect of seeming to reinforce the long-discredited view that France's colonial intent was entirely altruistic and conceived with no other objective in mind than to spread the blessings of the French revolution to the rest of humanity. But it should be added that the ambiguity of Laurens's conclusions in no way detracts from the very real merits of his rigorous analysis. His book largely succeeds in demonstrating that the evolution of the Orientalist strand in the French Enlightenment idea of progress produced a significant change in the West's perceptions of the Muslim East, replacing the old polemical image of a hostile and alien infidel with a more tolerant understanding of the world of Islam. It also serves to remind us that from the standpoint of the history of ideas the *longue durée* can sometimes suggest a useful corrective to the hasty conclusions reached by practitioners of *l'histoire événementielle*.

ALAIN SILVERA

Bryn Mawr College

JACK R. CENSER and JEREMY D. POPKIN, editors. *Press and Politics in Pre-Revolutionary France*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xiii, 252.

As the coeditors' preliminary comments suggest, the essays in this collection have been brought together with several related purposes in mind. A brief preface argues that the Bourbon government's treatment of the press (*de facto* if not *de jure*) is wrongly labeled "authoritarian" and that political journalism after 1789 owed much to practices sustained under the Old Regime. A jointly written first chapter, which surveys historiography, links studies of the press to works on "livre et société" and the "social history of ideas." Setting the history of the journal against that of the book, it deplores the relative neglect of the former and boldly asserts that the journal played a central role in prerevolutionary politics. (Oddly enough, the editors twice mention [and twice mistitle] the volume in the *Evolution de l'Humanité* series that deals with the book but leave unmentioned the volume

in the same series that deals with the journal. Lucien Febvre and Henri-Jean Martin's *L'apparition du livre* [1958] is thus mistitled *L'avènement du livre* [pp. 4, 8]; Georges Weill's *Le journal: Origines evolution et role de la presse périodique* [*L'évolution de l'humanité* XCIV] [1934] is undeservedly overlooked here [and in many other recent histories of the press.] A more modest common claim is also set forth and upheld by most of the essays that ensue: that periodical coverage of political contestation was less muted and more extensive than hitherto assumed. Although the editors do not say so, I find it significant that lively coverage of French politics invariably came from journals published abroad.

The first essay, to be sure, deals with a journal published within France. But, as Nina R. Gelbart shows, few direct references to politics could be smuggled into the *Journal des Dames*. Sketching the fluctuating fortunes of this specialized monthly during its twenty-year existence from 1759 to 1778, Gelbart provides memorable vignettes of its three, equally indomitable, albeit markedly diverse, female editors. She describes how they struggled (as their twentieth-century counterparts still do) to supply "women's pages" with serious, thought-provoking fare. Under the cover of frivolity and with the aid of highly placed male protectors, the journal succeeded in delivering some antiestablishment messages before the censors stepped in. The silencing of this journal is passed over so quickly that it almost escapes notice. One wonders if the "authoritarian" label has not also been too briskly dismissed. In any event, the *Journal's* circulation was so small, its life so curtailed, its political coverage so indirect that it offers scant support to the editors' preliminary claims.

These claims are best upheld by two subsequent essays, both analyzing the contents of the *Gazette de Leyde*. Jeremy D. Popkin, who is writing a book on the long-lived biweekly, which was founded in the seventeenth century by Huguenot refugees, describes its portrayal of French politics from Louis XVI's accession to the Bastille's fall. Carroll Joynes offers an equally solid background piece on how the *Gazette* had earlier handled the escalating "refusal of sacraments" affair. Both underline the *Gazette's* uniquely detailed coverage of domestic French politics and its consistent opposition to "ministerial despotism." But when dealing with whether and how this affected royal policies the two accounts diverge. Popkin takes for granted that French governments "had to acknowledge the influence of the press"; "even the King had to work at cultivating [a] . . . public image" (p. 130). Joynes finds it unsurprising that "the monarchy was unable to see what was happening" and asserts that "neither clergy nor crown realized that the

battle was no longer restricted to Versailles and . . . the Palais de Justice" (p. 167).

Jack R. Censer's penultimate essay leaves this contradiction unresolved. Devoted to the *Courier d'Avignon's* depiction of English politics from 1773 to 1783, it contains perceptive comments about foreign reporting as a special genre. But it introduces unnecessary complications by taking up a different journal's treatment of a different country. Had Censer dealt with the English news conveyed by the *Leyden Gazette*, linkage with previous essays could have been better sustained. The decision to rely exclusively on the *Courier's* London correspondent also entails excessive dependence on a single cautious source. This may help explain why the *Courier's* coverage of English politics during the American revolution, as conveyed by Censer, seems so much less lively than does the *Gazette's* coverage of French politics, as described by Popkin and Joynes.

The muted reports carried by the *Courier's* London correspondent also work at cross-purposes with the thesis proposed in Keith Baker's final essay. Although he makes glancing references to previous chapters, Baker is concerned with a set of issues different from any previously raised. Writing about the development of public opinion as an intellectual construct, he stresses the fearful reaction of French statesmen and writers to the divisive politics of Georgian England. Distancing themselves from English publicity and factional strife, the French defined public opinion as a "rational consensus untroubled by the passions of willful human action" (p. 246).

Baker pays considerable attention to French views of Georgian England but says little about press coverage of Bourbon France. Was there no relationship between the way the French construed "public opinion" and the circulation of newspapers produced abroad? One wishes the editors had supplied a concluding chapter to tie up loose ends. But they have given us so much—enough to make their collection required reading for anyone concerned with the eighteenth-century political press—that it seems ungrateful to ask for more.

ELIZABETH L. EISENSTEIN,
EMERITA
University of Michigan

VIDA AZIMI. *Un modèle administratif de l'ancien régime: Les commis de la ferme générale et de la régie générale des aides*. Paris: Centre National de la Recherche Scientifique. 1987. Pp. xii, 176.

Given the importance of the prerevolutionary French civil service, any study of it ought to be

welcomed. Unfortunately, Vida Azimi's study of the clerks of the Fermes and the Aides is too unstructured for a warm welcome. It lacks precisely the techniques that, in the preface, Jacques Lelièvre tells us Azimi has brought together to give a complete picture. Sociologists, historians, and jurists will all regret the failure to use the models and theories that might have redeemed the author's detailed archival work. The book draws heavily on original sources, mainly from the Archives nationales G series but is less convincing in its use of more recent work.

The book is divided into two sections. The first deals with structural matters: the location of the provincial offices of the Fermes and Aides, their hierarchies, and their *modus operandi*. The second looks at staff: their backgrounds, their entry into the service, and their motivation. There is also a coda following clerks into the revolutionary era, drawing on an annex of extracts from lists of ministerial employees drawn up for the post-Fructidor purge.

Azimi concludes that the twenty-eight thousand staff of the Fermes and Aides were a classic case of bureaucratology. They were hierarchical, over-centralized, and suffocated with paper. Employees were drawn to the job by prospects of a good pension, were resistant to discipline, and abused their power and position. She also confirms existing portraits of civil servants as usually being young direct entrants originating in northeast France.

The problem is that all this is not systematically organized, analyzed, or compared. Sociologists will lament the absence of reference to Max Weber's definition of bureaucracy, just as they will query the statistical methods and the paucity of social analysis. Historians, too, will be puzzled by the limited use made of scholarship on French social structure. Moreover, little is said of the relationship between staff and public, especially where the *rats du cave* are concerned. Jurists also will regret the absence of systematic consideration of the employee's *statut* even though the demand for a proper career structure was clearly evident before 1789.

Without these critical methods the book becomes an accumulation of lists and other pointillist presentations of fact. When one would like to know more, as with the *cautionnement* demanded of staff, it is not there. More significantly, the lack of structure means that the evidence presented does not always justify the interpretations placed on it. Azimi believes her research proves the case for administrative continuity between *ancien régime* and revolution. Yet the evidence emphasizes the way staff were imprisoned by the old honorific social system, its patronage, family connections,

and property rights. From this resulted their disillusion with the Fermes and their desire for a merit-based service. Whether the *ancien régime* could have delivered this is unclear. Yet, paradoxically, by concentrating on eighty-six employees who worked in the directorial ministries (a figure that is an underestimate), she plays down the contribution they did make to administrative development. One hopes that some of these weaknesses are remedied before the appearance of the larger work on the late eighteenth-century civil service of which this apparently is an offshoot so that Azimi's devoted labors in the archives are not wasted.

CLIVE H. CHURCH
University of Kent

MURRAY FORSYTH. *Reason and Revolution: The Political Thought of the Abbé Sieyès*. New York: Leicester University Press or Holmes and Meier. 1987. Pp. 248. \$49.50.

Seeking to dispel Sieyès's image as "the quintessential constitution-monger for all seasons" (p. 167, paraphrasing Edmund Burke), Murray Forsyth portrays him as "a master spirit of the age of the French Revolution" (p. 224). He argues that Sieyès created a "self-subsistent theory of politics" (p. 2) pervaded by "unrelenting logic" (p. 3) designed to justify representative government. Unfortunately, Sieyès was unwilling or unable to finish a work unless under pressure to do so, and important parts of his system survive only in fragmentary form.

In this book, by integrating published and unpublished material, Forsyth imposes structure on Sieyès's ideas and carefully guides the reader through their intricacies. The first chapters describe his thought processes and insist on his originality. The following ones analyze Sieyès's system: his concept of nationalism, his theory of revolution and the need therefore, and his prescription for creating a French nation. The author concentrates on political issues: Sieyès's definition of personal liberty, his arguments for representative (as opposed to democratic) government, and his successive plans for a central government. Such historically significant topics as the church, education, the press, justice, and the army are discussed in a single chapter.

Finding a "strong underlying continuity in his ideas" on government from Sieyès's student days through Brumaire (p. 167), Forsyth denies that events significantly influenced his thought. He admits, however, that the Jacobin era was a watershed; thereafter Sieyès was obsessed with protecting France's representative system from anarchy

and despotism. As a result, by 1799 Sieyès's constitutional proposals became excessively subtle and cumbersome.

In conclusion Forsyth credits the abbé with transforming abstract concepts into practical, concrete ideas and with recognizing representative government as a principle of rather than a deviation from democracy. Describing Sieyès as an important link between the Enlightenment and modern liberalism, Forsyth traces Marx's appeal to the proletariat to Sieyès's call to the Third Estate (pp. 81, 223).

This book offers a coherent picture of Sieyès's political ideas. But to appreciate Forsyth's theory the reader must know revolutionary history; the author provides only a rudimentary description of Sieyès's career, and his biographical sketch raises more questions than it answers.

Historians will be disappointed with this work, wishing that the author, having read archival material inaccessible to most readers, would give details about his subject's attitudes toward significant nonpolitical issues. Incidental comments that imply that Sieyès was impatient with details and impressed with his own accomplishments are tantalizing. But Forsyth fails to provide further insight into the contradictions of a thinker who, on the one hand, wished to re-create society on a systematic, orderly basis, but who, nonetheless, failed to organize his ideas during his long life, twenty-one years of which were spent in retirement.

These criticisms presumably are less important to political scientists than to historians. Yet, although historians might deny Sieyès the title "master spirit" of the revolutionary era, the author's careful, almost didactic, exposition enables them to appreciate the orderliness of Sieyès's thought and to understand its content more clearly.

RUTH F. NECHELES-JANSYN
Long Island University,
Brooklyn

JEAN-PIERRE JESSENNE. *Pouvoir au village et révolution: Artois 1760-1848*. Lille: Presses Universitaires de Lille. 1987. Pp. 308.

In his relentlessly systematic study of Artois, Jean-Pierre Jessenne undertakes an empirical critique of the idea (which he identifies with, among others, Suzanne Berger and Eugen Weber) that French peasants remained subordinate and unpolitical until the late nineteenth century. He argues that "in northern France, a peasant interest in collective control was clearly articulated by the end of the eighteenth century and consequently the

revolutionary struggle for power in the village strongly affected later developments" (p. 11). Depending on the relations of the agricultural elite to outside powers, he argues, that demand for autonomy variously led the communities of different regions to resist, support, or compromise with the revolution and subsequent national regimes.

To pursue the study of local power, Jessenne singles out 149 of Artois's 750 villages for an analysis of local officeholding from 1770 to 1799, follows the rural elite in 75 of those villages to 1848, and looks closely at the interlocking of social structure and officeholding from 1770 to 1848 in 20 villages. A report of the first two analyses, essentially chronological, occupies the book's first half, and the treatment of village structure, organized analytically, takes up the second half. Jessenne's concentration on successive catalogues of officeholders and the absence of events except as occasional illustrations mean that the first half jumps abruptly from period to period and spends quite a few pages on questions of evidence; still, the evidence serves its purpose well. The book's second half reports study after study (many of them Jessenne's own) of property holding, demography, marriage, social mobility, and other indications of the lives and positions of the principal class from which officeholders came. Working almost exclusively in the departmental archives of the Pas-de-Calais and drawing on the dense network of theses, memoirs, and publications produced by the lively group of historians who have worked at the University of Lille over the last two decades, he is able to cover the whole province without spending a lifetime on the effort. He also breaks with the *gros pavé* model of French rural studies by packing all of his analysis into a mere 260 pages of text, plus 40 dense pages of appendixes. Throughout his book, Jessenne states the issues with exemplary clarity and brings his sources systematically to bear on the issues. Jamming all that material into so few pages produces a fragmented but rewarding volume.

Jessenne's village studies reveal a region where nobles and clerics held a little over half of all the land as against one-third for the peasantry, where 60 to 80 percent of all farms consisted of fewer than five hectares (which implies that a similar majority of farm operators worked part-time for others), where one-quarter of household heads worked primarily as agricultural wage laborers, where taxes, tithes, rents, and feudal dues took a relatively low 30 percent of the income from leased land, and where one-fifth of the rural land went on sale with the revolutionary seizure of church and noble properties—in short, where agricultural capitalism was well advanced by 1770. In such a region, Jessenne finds that large lease-

holders (*fermiers*) dominated local politics, but only within limits set by their noble and ecclesiastical landlords. The revolution, by sweeping away the privileges of those patrons, threatened the leaseholders' power. They survived the challenge, however, as a class, if not as a particular set of individuals. Many officeholders lost their posts during the revolution's early years, especially when the community was already at odds with its lord, yet their replacements came disproportionately from the same class of comfortable leaseholders. The struggle of wage laborers and smallholders against the *cogs de village* that Georges Lefebvre discovered in the adjacent Nord was less intense, or less effective, in the Pas-de-Calais. Although large farmers, viewed with suspicion by national authorities, lost some of their grip on public office during the Terror and again under the Directory, they regained it later and continued to rule their roosts through the middle of the nineteenth century. By that time, nobles and ecclesiastics had lost much of their capacity to contain local power holders, but manufacturers, merchants, and other capitalists had taken their places.

CHARLES TILLY
New School for Social Research

TOBY A. APPEL. *The Cuvier-Geoffroy Debate: French Biology in the Decades before Darwin*. (Monographs on the History and Philosophy of Biology.) New York: Oxford University Press. 1987. Pp. 305. \$35.00.

This volume is one of the first in a new series of monographs, on the history and philosophy of biology, to be published by Oxford University Press. Judging from this example, I predict that the series will be a deserved intellectual success, for *The Cuvier-Geoffroy Debate* is a first-class piece of work. The author covers an area of science's history about which we have long known something, but not enough; she balances judiciously the straight science and all of the other factors, especially the politics of biology; and, above all else, she tells a rattling good yarn about the clash between two of the greatest prima donnas in the story of nineteenth-century France.

Toby A. Appel's study centers on the rival visions of the great father of comparative anatomy, Georges Cuvier, and his fellow anatomist, the transcendentalist Etienne Geoffroy Saint-Hilaire. Initially friends, indeed Geoffroy (although slightly younger) had introduced Cuvier to Parisian science and at first they even lived together, the two biologists took increasingly divergent paths. Coauthoring papers at the time of

the revolution, by 1830 they had broken into open and violent conflict, entertaining and dividing all in their audience at the Academy of Sciences. It is oft-told how the final bitter dispute centered on Cuvier's static view of life's processes against Geoffroy's evolutionism, and it is not Appel's aim to deny truth to this belief. But she amply shows that it is only part of the full story and that for full understanding we must dig more deeply into the antagonists' pasts.

Cuvier was a teleologist. For him, following Aristotle and Kant, the key to the understanding of organic nature lay in its directedness, its sense of purposeful organization. This insight Cuvier encapsulated in his well-known thesis of the "conditions of existence," arguing that organisms cannot just exist in any old way—they must be constructed according to constraining principles, which ensure that they work toward their ends of surviving and functioning. Translated into action, the conditions became the doctrine of the "correlation of parts," in which Cuvier claimed that simply specifying one part of an organism determined definitively all other parts. Crucial also to Cuvier was his four-part division of animals (into *embranchements*) with members of one kind (such as the vertebrates) having no essential similarities with members of another kind (such as the mollusks).

For Cuvier all parts had to have a purpose or function, and it was this claim that Geoffroy seized on, at first indirectly and then more and more directly. Like the *Naturphilosophen* in Germany (although Appel claims independently), Geoffroy uncovered much evidence of the widespread existence of adaptively pointless isomorphisms between animals, where parts in one would be twisted and transformed to perform quite different functions in others. These similarities (later christened "homologies" by the British anatomist Richard Owen) seemed to Geoffroy to be indicative of principles of organization and unity quite alien to Cuvier's picture of harmonious teleology. Finally, the underlying oneness of animal existence drove Geoffroy to evolution, and Cuvier (who Appel suggests was more influenced by religion than most have suspected) took strong exception, arguing with all the intellect and sarcasm at his disposal (both of which were considerable) that change is empirically false and conceptually impossible.

Ironically, as Appel shows, given the subsequent triumph of evolutionism, in many respects Cuvier gained—rightfully—the upper hand in the debate. But, as Appel also shows quite convincingly, there was much more to the debate than dry facts, and she brings to life early nineteenth-century France as she takes us through the scheming and

politicking of the scientists. Unless things have changed very drastically, it will be hard for the reader ever again to regard scientists without a touch of cynicism.

I suspect, with reason, Appel would argue that her story begins in the past and ends in the past. I wish, however, that today's historians of science did not have such a terror of the sin of whiggishness—of seeing the (imperfect) past purely as a stage toward the (perfect) present. Then might Appel have concluded her book by telling how the debate between Cuvier and Geoffroy, between the enthusiast for function and the enthusiast for form, is being replayed by today's evolutionists, between, for example, Richard Dawkins and Stephen Jay Gould, likewise respective enthusiasts for function and form.

MICHAEL RUSE
University of Guelph

PATRICIA MAINARDI. *Art and Politics of the Second Empire: The Universal Expositions of 1855 and 1867*. New Haven: Yale University Press. 1987. Pp. viii, 247. \$40.00.

For two centuries before the middle of the nineteenth century the art world of France was dominated by the Académie des Beaux Arts and the government, the academy as the arbiter of taste and the government as the principal patron. By the end of the nineteenth century it was dominated by the market and the bourgeoisie. In her study Patricia Mainardi shows when and how this momentous change came about and offers her explanation of why it occurred.

The academy had favored—and the government had chiefly purchased—large, carefully crafted paintings of historical, religious, or mythological subjects. Landscapes and genre painting were denied official favor. For the government of Napoleon III, however, styles and schools were less important than political loyalties, and in 1855 it opened the art exhibition of the Universal Exposition to all living artists. It sought to impose no particular style but to assure representation of all. In succeeding years the imperial regime gradually cut back the power of the academy over exhibitions and awards and made the salon juries more democratic. In 1867 it sponsored both an art exhibition as part of the Universal Exposition and a regular salon of recent works. Together, Mainardi holds, these two shows marked the death of history painting in France, the triumph of genre painting, and the end of the official, academic world of art.

The ultimate cause of this upheaval, according to Mainardi, was the rise of the bourgeoisie to

affluence and leadership in France. As the roles of the academy and the government diminished, all schools and styles had to compete for purchasers, and the bourgeoisie, through the medium of the market, became the new arbiters of taste. They preferred genre paintings, and the democratized salon juries reflected their tastes.

My necessarily brief summary does less than justice to the richness of the author's knowledge, her skillful interweaving of art history with political and economic history, and the cogency of her argument. Her book is illuminating and largely convincing. I do, however, find her class explanation a bit too facile, incompatible with what historians now know about the complexities of the patterns of wealth and power and the realities of the market in nineteenth-century France. The directing class during the Second Empire and the early Third Republic was not exclusively bourgeois, nor was the art world peopled exclusively by bourgeois. The aristocracy still controlled much of the nation's wealth, and aristocrats were still buying art. So, too, was the state, whose representatives did not necessarily follow the taste of bourgeois buyers. Provincial museums were in the market, too, and their acquisitions were commonly determined by committees of local notables, who were not exclusively bourgeois.

This reservation is not meant to diminish the value of this excellent book. It is a delight to read, rewarding to every manner of historian, and Yale University Press has given it a beautifully composed and illustrated form, worthy of the author's text.

DAVID H. PINKNEY
University of Washington

MICHELLE PERROT. *Workers on Strike: France, 1871–1890*. Translated by CHRIS TURNER. New Haven: Yale University Press. 1987. Pp. 321.

This book offers the nonspecialist a unique entry into the scholarship of one of the most influential nineteenth-century French social historians. Michelle Perrot is a pioneer in the effort to get beneath the history of French labor ideology with an ambitious and still unequaled study of about three thousand strikes in the “adolescent” years of French industrial labor, between the Commune and the upsurge of organized labor after 1890. Although, by contemporary standards, her quantitative efforts are modest—primarily descriptive without seriously addressing theories of collective action—they provide us insights into the morphology of the strike. Her study, based on an astonishing range of archival and printed sources, also anticipated the present preoccupation with the

language of social movements in her influential analysis of the strike as an expression of collective memory and aspiration.

Rather than treat the strike as a variable to be inserted into an economic model, she presents the reader with the contours of the strike from its beginning to its conclusion. In this abridgment especially, the strike is a "classic piece of urban theatre" (p. 12). In this relatively unorganized world of labor (72 percent of her strikes lacked union direction), Perrot finds largely spontaneous and localized strikes with leaders characterized by their marginality, youth, mobility, charisma, transience, and even criminality. These "primitive rebels," she argues, only slowly gave way to the more disciplined, even bureaucratic, revolutionaries. The "general strike," unlimited by duration or extent, gradually became the preferred weapon of industrial action in the 1880s. This long predated well-known theories of anarcho-syndicalism. Unlike the modern, pragmatic "partial" strike, which targets employers and seeks specific goals, the nineteenth-century general strike had quite different purposes. Strike demands were predominantly economic, often defensive, and traditional in their assertion of the right to work. Yet the wider purpose of the strike was psychological or symbolic. In a highly detailed study of strike songs, speeches, demonstrations, and meetings, Perrot finds a complex of meaning. Strikes gave vent to anger in mock violence. Participants used a traditionalist language of, for example, hanging industrial lords from gas lamps and verbal or symbolic attacks on "republican lords" with their exploitative *bénéfices*. The strike also meant the liberating "freedom of the street" and transcending the isolation of the industrial division of labor.

Finally, Perrot offers a brief but tantalizing analysis of the still primitive collective bargaining process, especially the roles and attitudes of employers and the state. She outlines the individualist perspective of employers, which impeded association and tended to guarantee a margin of success to strikers who could hold out (at least for a few weeks). Increasingly, the state was willing to intervene in strikes. With much ambiguity, authorities gradually saw their role as a mediating one rather than merely defending property and the "freedom to work."

This book is an abridged translation of basically the second volume of *Ouvriers en grève: France 1871-1890* (1974). This English version of Perrot's masterpiece has the virtue of being well translated and providing a focus that appeals to the contemporary reader in language. Its claims are refreshingly clear and modest in both the quantitative and "literary" dimensions of the book. Much, of course, is left out, beyond the notes and

full bibliography. The linkages of strikes to political trends and to other forms of protest, the history of the French labor movement, and the detailed analysis of the evolution of strike demands are absent from this translation.

The greatest virtue of this book, however, is that it offers so clearly both a methodological perspective and an agenda for historical research. Although historians already have learned much from Perrot's insights into the social origins of French working-class culture and labor relations, few have followed through on her analysis of working-class action after 1890. How, for example, did labor leadership change, and the triangle of state, employer, and worker actually evolve? Perhaps this most accessible book will stimulate this research.

GARY CROSS

Pennsylvania State University

CHRISTOPHER GREEN. *Cubism and Its Enemies: Modern Movements and Reaction in French Art, 1916-1928*. New Haven: Yale University Press. 1987. Pp. 325. \$60.00.

In reviewing the cubist legacy from the postmodernist vantage point, art historians have been increasingly convinced that the concerns of formalist critics two and three decades ago, such as those of the American Clement Greenberg, need to be reevaluated. Specifically, critics saw early cubism as part of the modernist battle to remove painting and sculpture from its social and cultural moorings and to view aesthetics in isolation. Christopher Green's subtle and richly descriptive study recaptures the numerous differentiations between cubist artists and other art styles over time, concentrating in particular on the 1916-28 period, after the first shock of Picasso's, Braque's, Gris's, and Leger's innovations had already been assimilated. Green also describes what he refers to as a "single discourse of culture which is far more wide ranging and longer lasting than the art and debates actually covered" (p. 3). But the satisfaction that comes from reading this work is that, although the author employs the currently fashionable concepts of "structures of discursive formations" or "hegemony and counter-hegemony" of cultural historians such as Michel Foucault and Raymond Williams respectively, he does not overburden his text with these suggestive formulations, which can obstruct as well as aid the reader. Rather, and to his credit, Green's narrative lodges these concepts in his descriptions. At the same time, he formally analyzes individual works as self-contained visual texts.

One of Green's major themes is surrealism's

challenge to cubism and both the critical and artistic rhetoric surrounding that challenge. He lucidly summarizes the differences: "the Cubists' refusal of the simply sensual, their promotion of the artist as the controlling centre of attention, and their insistence on the primacy of the art-work as a fixed and self-sufficient entity. Those major Surrealist themes that most obviously presented such challenges were the erotic, the theme of inward vision and, linked to it, of impotence, and then themes both of metamorphosis and of liberation" (p. 283). The "enemies" in the title of the book, then, refer not only to conservative opposition to cubism but to what Green defines as "radical [aesthetic] alternatives" to the style, such as dada, surrealism, and constructivism. He correctly dismisses interpreting the history of modernism as a linear progressive succession of styles. He effectively succeeds by his very detailed description of the numerous debates carried on by the artists and their critics during the 1920s. Earlier accounts of cubism tended to downplay its later history (that is, after World War I) as aesthetically derivative and producing few innovations.

Green's analysis of cubism is firmly within the best scholarship of the last two decades, which according to Patricia Leighton is characterized by a loss of interest in "line, color, and form" and a "reconsideration of iconology, allusion, and other levels of content." Given the shift in scholarly focus from internal analyses to a wider review of modernism's entire milieu, modernism itself as a category of analysis has also undergone significant reevaluations, of which Green's fine monograph is a recent example.

MARION F. DESHMUKH
George Mason University

RICHARD I. COHEN. *The Burden of Conscience: French Jewish Leadership during the Holocaust*. (Modern Jewish Experience.) Bloomington: Indiana University Press. 1987. Pp. xii, 237. \$27.50.

In the early years of World War II, pride in French liberal traditions prevented native French Jews from comprehending that the legal emancipation they had enjoyed for one hundred fifty years was seriously threatened. Only after an unfortunate delay did official Jewish leadership realize that racist legislation was more than a temporary aberration. Immigrant Jews, on the other hand, had experienced a side of France that permitted fewer illusions. The first targets of this discriminatory legislation, they were also the first to recognize fully the dangers that Jews in France faced under the Vichy regime.

Richard I. Cohen's important contribution to

the study of French Jewry during the Vichy years provides a necessary corrective to less scholarly earlier writings on the topic. Meticulously researched and objectively argued, the volume avoids the pitfalls of passion and the tendency to see wartime native Jewish leadership as monolithic in goals and tactics. Cohen provides a balanced account of the work of the Union générale des israélites de France (UGIF), which operated as two separate institutions in the north and south.

Often accused of collaboration, the leaders of the UGIF faced impossible decisions concerning the Jewish communities of France, whose "welfare" was officially in their charge. Aryanization of Jewish businesses had caused a suddenly high rate of Jewish poverty. Although well-intentioned, these men were ill-prepared to make appropriate choices, for they were not privy to full information about official intentions and policy. Nor is it correct to suggest that the native French Jews who were selected for positions of leadership within the UGIF were all simply politically naive. Cohen demonstrates their efforts to combine underground illegal activities with a facade of official compliance, in an attempt to achieve the maximum benefit.

By a decree of January 9, 1942, the UGIF was established, and the northern division began to function immediately. At first it proceeded in an exclusively legal manner. After the deportations of that summer, however, UGIF-North realized the extent of French collaboration and began to tolerate and even to engage in illegal activity.

UGIF-South began activities only in May 1942. When Vichy began to accede to German requests to deport foreign Jews from the unoccupied zone, and especially with the January 1943 deportations from Marseille, UGIF-South reacted by allowing the independence of its constituent bodies, which was tantamount to authorizing illegal activity.

There was little common action between immigrant and native Jews. At first, immigrant Jews were rebuffed as they sought to cooperate with the natives. Later the situation reversed, as immigrants doubted both the desire of the natives to protect the immigrant community and the wisdom of their political judgment.

Immigrant suspicion was fed after the summer of 1943 by the refusal of UGIF-North to abandon doubtful activities, such as overseeing the camp at Drancy (the internment camp near Paris from which most of the deportations took place) and the maintenance of homes for the hundreds of children whose parents had been deported. Resistance leaders argued correctly, but in vain, that the children's security required that they be dispersed.

In refutation of postwar charges of collaboration, Cohen demonstrates that UGIF leaders dis-

regarded their personal interests: "The UGIF leaders maintained their moral integrity: with their councils' support, Bauer refused to deal with police affairs, and Lambert forcefully protested against the radicalization of German policy. Their opposition led to their arrest and deportation" (p. 190).

Yet Cohen does not romanticize the accomplishment of the UGIF. Painfully aware of the tragic situation in which wartime Jewish leaders attempted to serve, he nevertheless suggests that they might have been more cognizant of their inability to protect Jews under their care: "Tragically and paradoxically, hundreds were eventually deported by the German police as they were being cared for by UGIF workers . . . Despite their efforts and often owing to their lack of foresight, the two councils . . . had failed those Jews by being overly loyal to the humane goal of serving the needy" (pp. 190–91).

PHYLLIS COHEN ALBERT
Harvard University

JACQUES ADLER. *The Jews of Paris and the Final Solution: Communal Response and Internal Conflicts, 1940–1944*. (Studies in Jewish History.) New York: Oxford University Press. 1987. Pp. xxii, 310. \$32.50.

When the Nazis conquered France in June 1940, they clearly intended to impose a "final solution" on all of its Jewish residents. The fact that the overwhelming majority of the nearly eighty thousand Jews in France who met their deaths in extermination camps were foreigners can be explained in large part by the complex relationship that developed between leaders of the Vichy government, German occupying forces, and the Jewish community between 1940 and 1944. The purpose of the present study by Jacques Adler is to explore the varied responses of immigrant and native Jews in Paris to the distinctive nature of the process of the Final Solution in France.

Adler's work is a welcome addition to the growing number of monographs on Jews under Vichy that have appeared in the past decade. Complementing Michael Marrus and Robert Paxton's study of Vichy's anti-Jewish policies, Adler's book concentrates on the conflicting strategies devised by Jewish community organizations to protect the livelihood, property, and, ultimately, the lives of their members. Although recognizing that European Jewry essentially was helpless to stop the Nazi juggernaut, Adler nevertheless is critical of many of the actions of native and immigrant leaders in the French capital. The author argues that the reactions of the two groups stemmed from their

contrasting self-images and material circumstances. French Jews—largely upper bourgeois, politically conservative, and respectful of the social order—consistently ignored danger signals because they were convinced that government anti-Jewish policies were directed only against foreigners. This did not prevent the Union générale des israélites de France (UGIF), the largely native-run organization established by the Vichy government, from actively aiding immigrants suffering from legal discrimination and persecution. Unlike Jewish councils in other occupied countries, Adler argues, the UGIF cannot be held responsible for the deportation of Jews. Its major failing was what the author calls "wishful thinking," an ideological myopia that prevented French Jews from giving moral support to foreign coreligionists in their time of greatest need.

Immigrants, on the other hand, were much more aware of the potential fate that awaited all Jews in France. Less committed to France and more militantly Jewish than natives, immigrant organizations refused to be bound by Vichy or German laws. By far the most active element were the Communists, whose strategy of armed resistance, Adler believes, would have saved more Jews if it had been adopted by the entire community. Yet immigrants were hampered by internecine political divisions and by the constant threat to their survival posed by periodic deportations. For all its support of illegal activity, the Comité Amélot—the chief immigrant organization until its dissolution in June 1943—continually was forced to make compromises in order to secure daily relief for victims. Although Communists had few such restraints, their total dependence on the French Communist party and insensitivity to the need for a specific Jewish response made them far less effective within the community.

Despite his criticism of the actions of Jewish leaders, Adler makes it clear that the issue of responsibility ultimately rests with the victimizer and not with the victim. There were no collaborators among French Jews, for it was the extreme moral dilemmas resulting from efforts to thwart or delay the Final Solution rather than willful acceptance of Nazi policies that paralyzed the community leadership. The Nazis continued to play on community misunderstandings and fears until the last days of the war. As late as July 1944, only three weeks before Liberation, neither the UGIF nor the immigrant-led United Defense Committee proved willing to help prevent the deportation of a trainload of children, the UGIF fearing that an active response would threaten its survival, the United Defense Committee concerned about maintaining communal unity.

Adler has done a superb job of historical re-

search, drawing on material housed in the major Jewish archives of France, Israel, and the United States. Such an exhaustive effort is bound to have lapses. In concentrating almost exclusively on Paris, Adler never clearly differentiates between the conditions and activities of Jewish organizations in Paris and in the Southern Zone, a significant issue at the beginning of the occupation and again after the mass deportations from the French capital in July 1942. One would have also liked more information on the personal reactions of French and immigrant Jews to the impending tragedy, an issue addressed by Yerachmiel Cohen in his recent work on the UGIF.

Despite these flaws, Adler's study is unlikely to be surpassed by future historical investigation. Given his own experiences as a young immigrant in Paris who saw his father deported and who later joined a Jewish Resistance group, the author's meticulous and cautious approach is all the more remarkable. The author is to be commended for writing a rare work on the Holocaust that combines objective scholarship with a keen sensitivity to the unfolding tragedy of European Jewry during World War II.

DAVID WEINBERG
Bowling Green State University

SONIA MAZEY and MICHAEL NEWMAN, editors. *Mitterrand's France*. New York: Croom Helm. 1987. Pp. xi, 253. \$65.00.

If a pattern has emerged in the numerous academic works devoted to the Mitterrand government in the Anglo-Saxon world, it reveals that British and American professors fall into what the French call the "décus du socialisme," those whose initial enthusiasm for France's most profound socialist experiment to date has ended in disappointment. In fairness to the contributors to the present volume, it must be said that they take great pains to evaluate the Mitterrand *Septennat* in terms of the ideals and aspirations the French Socialists articulated prior to their electoral success in 1981. These are recalled in Michael Newman's conclusion to the volume: the Left wished to improve the living standards of the lower classes, democratize French society, provide equal opportunity for all (with particular attention to the rights of women), and carry out a progressive foreign policy of peace and economic assistance to the Third World. The individual authors demonstrate, each in his or her area of expertise, the failure of the Mitterrand government to live up to its ideals. If there is a broad measure of agreement on the dimensions of this historic failure, the authors do reveal some differences as to its causes. Some place greater

emphasis on the internal and external constraints under which the Socialists labored. The best they could do, Peter Holmes insists, was to carry out "radical capitalist reforms." Others conclude, with David Hanley, that the Parti Socialiste (PS) was never as radical as its rhetoric of the 1970s or alliance with the Communists suggested. The PS was always more or less a social democratic party "like the others" in Western Europe. Little wonder that by 1983 the slogans of *autogestion* and "break with capitalism" had given way to more reassuring terminology based on "modernization" and social justice.

The other contributors flesh out this analysis. Martin Rhodes notes that the Auroux laws, which were meant to democratize French industrial relations, had little effect in practice except to weaken the traditional structure of French labor unions. Doreen Collins shows that initial welfare benefits extended to the poor were rapidly curbed in the interest of restoring financial stability, while in attacking the women's question or early retirement the Socialists were prisoners of outmoded concepts appropriate to the 1960s. Decentralization, Sonia Mazey writes, was indeed "la grande affaire du Septennat." But, when the smoke had cleared and municipal powers had been formally transferred from prefects to mayors and presidents of department councils, most of the French noticed little difference in their personal lives; local politics remained very much dominated by traditional elites. Foreign policy, David A. L. Levy argues, was initially intended to improve French relations with the United States and demonstrate moral disapproval of the Soviets for their actions in Afghanistan and Poland, while emphasizing the welfare of the Third World. But by 1983 Mitterrand was again pursuing detente with the Soviet Union and closer ties with the European community. Defense policy, Neville Waites reminds us, remained geared to the nuclear deterrent, while nothing was done about the destabilizing effect of French arms sales abroad. Even in cultural policy, where the Socialists doubled government expenditure, Jill Forbes finds the pursuit of business as usual. Mitterrand, like his predecessors, showed a preference for grand projects of centralized ostentation, like the Musée d'Orsay, the Museum of Science and Industry at La Villette, the remodeling of the Louvre, and the Opéra de la Bastille. Regional projects received increased financing but little attention.

The authors have made their case, as others have done before them. But they have also vastly exaggerated it. The long-term impact of the nationalizations, which gave the French government control of 23 percent of industry and 92 percent of bank deposits, cannot be lightly dismissed with

the observation that operations continued much as before. The Mitterrand government's inability to come up with a coherent industrial policy perhaps obscures the fact that alone among the major Western powers France is now equipped one day to do so. The Auroux laws similarly opened up new perspectives in labor relations, most of which have yet to be realized. Decentralization coincided with a rightward trend in the electorate, which may have hidden the deeper transformation that underlies that reform as well. The editors may also be faulted for their choice of subjects. There is no mention of the defense of civil liberties by Minister of Justice Robert Badinter, who also carried out the abolition of the death penalty and reform of the prisons. The effect of Communist participation in the government also is ignored, despite the apparent permanent alteration of the political landscape as manifested in the party's rapid decline. This collection will be read with profit. But one may doubt that the current tendency among academics to denigrate Mitterrand will be sustained as some temporal perspective on his first *Septennat* is gained.

IRWIN M. WALL
University of California,
Riverside

IGNACIO ATIENZA HERNANDEZ. *Aristocracia, poder y riqueza en la España moderna: La Casa de Osuna siglos XV-XIX*. Madrid: Siglo Veintiuno de España. 1987. Pp. xiv, 447. 1,415 ptas.

Despite the many assertions about the aristocracy's role in the Spanish Old Regime, surprisingly little research has been done on this group. Ignacio Atienza Hernández has thus performed a valuable service with the publication of his study of the house of Osuna, an entity that included not only the duchy of Osuna but also all the other titles, lands, and jurisdictions held by the dukes. After a general discussion of the institutional, political, and social world of the Castilian magnates and a clear account of how the fabulous Osuna holdings were assembled, Atienza provides in the three succeeding chapters an analysis of significant areas of grandee involvement in Castilian life. First, he describes how lordship provided jurisdiction over the kingdom's fiscal, human, and material resources. Then he uses his Osuna material to explain the relationships between the aristocracy and the crown. Finally, he outlines the types of property held by the house and the uses to which it was put by the dukes. In closing, Atienza recounts the bankruptcy of the house that dispersed its holdings.

For most historians Atienza's treatment of the

power exercised by a major aristocratic house will be the book's greatest attraction, but the key to understanding grandee influence is the author's explanation of the nature of a noble's estate, since the dukes did not directly exploit the material resources to which their titles gave them access. Wealth came from rents paid by those who actually worked the productive patrimony of the house and from a multitude of revenue sources associated with various institutions and offices over which the dukes of Osuna had jurisdiction. Yet, although the size of the house's patrimony and the number of its jurisdictions provided the dukes with a significant role in the kingdom's administration, the power of action that this role allowed was often extremely limited. For one thing, the dukes exercised not private but public power delegated from the monarchy, and, thus, ducal commands and decisions could be appealed to a royal jurisdiction, like the Chancillería de Granada, where crown officials could exercise control over the dukes' actions and where they were subject to the expenses of defense. Indeed, expenses were an ever more prominent concern. The core lands and jurisdictions had been acquired in theory as rewards for various "services" to the crown, and such support for the monarchy was expected to continue. Increasingly, the house fell into serious debt as the dukes borrowed on the rents and fees of their entailed estates to the point where the seventeenth-century monarchy had to intervene directly in their administration. The type of political independence that was often enjoyed by the great medieval warrior-nobles escaped their successors, who were increasingly bound to the monarchy by a multitude of institutional and financial relationships. Although the nineteenth-century disentanglement of the house increased the dukes' real property and direct political influence, the inherited financial difficulties led the family down a slide toward bankruptcy.

There has been a tendency to think of the aristocracy as a group whose members were capable of dictating to the monarchs of the Old Regime, turning the crown into a servant of noble interests. Atienza demonstrates that no head of the house of Osuna was in a position to exert such pressure on any early modern king, but, in his challenge to traditional views, he sometimes overlooks his own excellent portrayal of the great complexity of monarch-magnate relations to grant greater power to the crown than it probably had. For example he appears to take a theoretical treatment of royal power as representing the actual understanding of the issue by the politically active. However, neither political theory nor politics is Atienza's subject, and this fine book is essential reading for an understanding of the

Castilian aristocracy during a central period of Spanish history.

J. B. OWENS
Idaho State University

J. VAN DEN BERG and G. F. NUTTALL. *Philip Doddridge (1702–1751) and the Netherlands*. (Publications of the Sir Thomas Browne Institute, Werkgroep Engels Nederlandse Betrekkingen, new series, number 8.) Leiden: E. J. Brill or Leiden University Press. 1987. Pp. 110. f. 40.

The life of Philip Doddridge, eighteenth-century dissenting minister, illustrates once again the interconnectedness of British church history with the continent of Europe, in this case particularly with the Netherlands. The seventeenth-century story is perhaps better known, with its three hundred or more English and Scottish ministers, many of them Puritans and Covenanters, who took refuge, traveled, or served in the Netherlands. Among Dutch churchmen interest in and connection with the British churches were also strong. Doddridge's career shows the strength of this historic Dutch-English link well into the eighteenth century. Doddridge was an influential dissenting pastor of Northampton and teacher in its academy. He wrote books and hymns.

The focus of this study by J. Van den Berg and G. F. Nuttall is "the reception of Doddridge" among the Dutch. Doddridge never visited the Netherlands, but his Dutch connections were nevertheless very visible. He read Dutch theology, several Dutch students went over to study at his academy, and some students of his likewise traveled to the Netherlands for study and for service in the English churches. Most important was the ministry of his books. Seventeen or more of his books were published in the Netherlands in Dutch translations. The appeal of Doddridge's books to Dutch readers lay in his warm piety and sincere devotion; devotees of the "nadere reformatie" no doubt read him eagerly. English and American Puritan books in Dutch translation had long helped fill this pious need. The writings of Isaac Watts and Jonathan Edwards were also greatly valued in the eighteenth-century Netherlands. In his correspondence and books Doddridge looked on the Netherlands "with the eyes of an English Dissenter" (p. 43). He prayed for revival, especially in such areas as Sabbath observance and family prayers.

Van den Berg and Nuttall present Doddridge in a clearly written and thoroughly researched book. It is a book of value to scholars of English dissent and of Dutch church history. Although it deals with a rather narrow slice of history, it serves to

enlarge our knowledge about the larger topic of the relationships between English and Dutch history. The Sir Thomas Browne Institute of Leiden University, which sponsors this publication series, is a center for the study of Dutch-English literary and historical topics.

KEITH L. SPRUNGER
Bethel College
North Newton, Kansas

JÖRN SIEGLERSCHMIDT. *Territorialstaat und Kirchenregiment: Studien zur Rechtsdogmatik des Kirchenpatronatsrechts im 15. und 16. Jahrhundert*. (Forschungen zur kirchlichen Rechtsgeschichte und zum Kirchenrecht, number 15.) Cologne: Böhlau. 1987. Pp. xi, 368. DM 94.

It is a paradox of our age that the more historians recognize the artificiality of periodization, the more we submit to it. Although many historians maintain interests in periods beyond the one in which they do their research (and the more recent the period, the more narrowly defined it tends to be), very few venture outside it in their writing. Scholars who courageously cross the boundaries and brave the fire of specialists deserve a more sympathetic hearing than is customary and, if they have done their work well, our thanks.

This is such a study, centered on the Reformation, the state, and ecclesiastical patronage, but reaching backward into the Middle Ages for understanding of the roots and evolution of the legal system of ecclesiastical patronage and laterally into the world of secular justice to explore connections with secular jurisprudence and with the personnel and operations of the Reichskammergericht. The several chapters constitute, as the subtitle indicates, studies that transcend the barriers between the Middle Ages and the Reformation, the ecclesiastical and the secular. This book has been long in the making, growing out of a dissertation finished at Constance under Horst Rabe in 1977. It is but part of a more ambitious project, to be complemented by a socioeconomic examination of the lower clergy of that part of the diocese of Constance that lay in Württemberg in the late Middle Ages and the Reformation.

One of Jörn Sieglerschmidt's most significant conclusions is that with respect to the benefice system the Reformation largely accomplished what the medieval church had not. For, although the reformers of the eleventh and twelfth centuries wished to establish a large measure of clerical control over patronage, their respect for established rights and custom (as well as political realities) led to compromises that guaranteed for founders and collators considerable effective con-

trol over appointments. The result of the Reformation was to limit their rights severely for two principal reasons. First was the new factor of confessional differences, in which the confession of the territorial prince dictated the confession of those who could be presented to him. The second tendency built on the late medieval collaboration between popes and princes to promote university men to benefices. The princes of the Reformation continued or renewed this secular trend, which worked to the disadvantage of the family and friends of collators, especially of aristocratic collators. Princely prerogative also largely triumphed, it turns out, at the expense of the Protestant principle of elections of ministers by congregations, which in fact achieved very little success. In terms of the quotidian workings of the church and its patronage system, then, the princes were the winners, the aristocracy the greatest losers.

In such a broad-ranging work it is to be expected that there are omissions (for example, Geoffrey Barraclough's old but still fundamental *Papal Provisions* [1935] is missing) and errors (such as an oversimplified understanding of the orders of the clergy [p. 33]). Much more disturbing is the fact that these important conclusions and insights are obscured and impeded by a German prose style that is peculiarly uncongenial. Even to someone who is habituated to what has been called the *Begriff*-stricken character of much German historical work, this study is particularly vexing. The problem is encapsulated in the term *Rechtsdogmatik*, which Sieglerschmidt uses in the subtitle and explains with considerable care (p. 4, n. 3), but which I still find pretentious, as I suspect most historians would. Perhaps social scientists, who are fond of such neologisms, will be pleased. That would be regrettable, for, while Sieglerschmidt has much to teach historians, social scientists are scarcely likely to listen to what he really has to say.

LAWRENCE G. DUGGAN
University of Delaware

WOLFGANG BEHRINGER. *Hexenverfolgung in Bayern: Volksmagie, Glaubenseifer und Staatsräson in der frühen Neuzeit*. Munich: R. Oldenbourg. 1987. Pp. ix, 533.

This is a major contribution to the mounting literature on witchcraft and its prosecution at the time of the great witchcraft. And, although originating as a doctoral dissertation, it is a worthy addition to the distinguished *Frühe Neuzeit* series, which includes contributions by scholars such as Peter Blickle and Heinrich Lutz. Eschewing older models, the author expressly follows Anglo-American scholars Alan Macfarlane and Erik Midelfort.

From Macfarlane comes attention to such things as ethnology and the social function of witchcraft accusations; from Midelfort's study of southwestern Germany, to which this is complementary, a stress on the contemporary understanding of the subject. And, although Wolfgang Behringer displays something of the quantitative bent of both Macfarlane and Midelfort, he seems to me more akin than is acknowledged to the English Annaliste Keith Thomas. Here one finds a similar function for religion and a similar catholicity or sweep. He cites, for example, Emmanuel Le Roy Ladurie's history of sunshine.

Contrary to some historians who still want to explain away the witch craze as an aberration alien to the great issues of the day (the "conspiracy of silence" of Eugenio Garin, as still evidenced in many textbooks), Behringer insists that it has a "central meaning." It was at the very heart of the three essential currents that he sees following one another in early modern times: the traditional magic of popular culture, the "new fanaticism" of the religious reform (on this terrain, primarily Catholic), and the secular rationality of reason of state. The first was not exactly discouraged by the emperor himself, Rudolph II, that "strange representative of high magic," with his retinue of magicians, much to the consternation of the Jesuits. Of course folk magic was eventually diabolized and criminalized. The new fanaticism is seen as portended when Peter Canasius, likened here to John Knox, created great stir in the 1560s with his acrid preachments and sensational exorcisms. Everywhere there was a new moral straitening, creating intolerable social tensions. And, then, the witch craze became a sordid chapter in the process of centralization, rationalization, and, ironically, "civilization."

But this can get ahead of the story, for its matrix is "a crisis of the late sixteenth century" and its "revolution of mentalities." This was composed of a constellation of things: chronic poor weather and crop failures, an agrarian crisis and price revolution, a proliferation of fortunetellers, magicians, and charlatans eager to profit from the crisis but vulnerable as its scapegoats, and, finally, the new religious rigor.

The witch craze was no respecter of religious confessions, but here, as earlier anticipated by Midelfort, the Lutherans eventually inclined to the more skeptical tradition of the medieval *canon episcopi*, moderating the prosecutions, while the Catholics went on to their most notorious excesses, as at Bamberg, Würzburg, and Eichstätt. I am not sure that the author has adequately harmonized all his statistics (pp. 40, 67, 68, 431), but, because of the likes of Bamberg, there were some three thousand executions in Bavaria overall. Interest-

ingly, in five years time there were more executions for witchcraft than in the previous five hundred. As recent figures suggest, those statistics are not unrepresentative of the larger difference between medieval and early modern prosecutions.

The scholarship is exhaustive, and the bibliography cites everything from Umberto Eco's famous novel to my own bibliography published in the *Journal of Popular Culture* (1971). Unfortunately, Joseph Klait's synthesis (1985), which helps illuminate the relationship between the witch craze and early modern centralization, was published too late for use. The only significant omission I detect is Jean Delumeau, *La peur en occident* (1978), which somewhat anticipates Behringer. But Behringer should be the standard on Bavaria for a century.

DONALD CHRISTOPHER NUGENT
University of Kentucky

DAVID SORKIN. *The Transformation of German Jewry, 1780–1840*. (Studies in Jewish History.) New York: Oxford University Press. 1987. Pp. 255. \$29.95.

David Sorkin's study of change in the German Jewish community is an innovative and generally convincing analysis of a well-studied but still controversial subject. The book sets out to explain the strange paradox by which the German Jewish community went to great lengths to integrate into general German society and yet remained distinctive and conspicuous. The subtly argued conclusion is that the very establishment of institutions to facilitate the change created a German Jewish subculture that, though invisible to its adherents, continued to foster a separate if parallel Jewish communal life.

Although Sorkin has read widely in the social history of German Jewry, his work tends to concentrate on the ideological implications of political and religious positions. Still, Sorkin does not merely go over the familiar ground of Enlightenment, emancipation, and religious reform in the conventional way. He is less guilty of exclusive concentration on Berlin than are most students of German Jewry. Although such familiar figures as Moses Mendelssohn and David Friedlander appear in the book, the main focus is on less familiar personalities, notably on the founders of the reforming Jewish journal *Sulamith* in provincial Dessau. The book is much interested in the sermons of the period and in the activities of German Jewish voluntary organizations.

Sorkin analyzes the complex connections between intellectual influences of the Enlightenment, the political and social demands of emanci-

pation, the changes in German society in the nineteenth century, and the resulting transformation of German Jewry. He often finds continuity where others have found sharp discontinuity, for instance, between traditional moralistic literature (*Musar*) and the criticisms of tradition by the Jewish Enlightenment.

Sorkin traces the convergence of internal Jewish intellectual desires for reforms with external demands by the architects of emancipation for regeneration of the Jews. Most Jewish leaders in the period seem to have accepted the demand that the Jewish community prove itself worthy of citizenship by changing its educational and social makeup. Sorkin sees the main tool of the proposed change in the Jews to be *Bildung* (which he translates variously as "education" or "formation") and the main model for integration to be the educated bourgeoisie (*Bildungsbürgertum*). Jewish integration was to a specific stratum of German society rather than to a cross-section of it.

This book is written in clear but academic prose. The argument is well constructed and sustained throughout the volume (except perhaps in the last two chapters). Despite the claims of the title, the period chosen marks only the beginning of the "transformation of German Jewry." The author himself admits that although the framework of change was in place by 1840 the "subculture" did not yet encompass the majority of German Jewry. The crucial impact of industrialization and urbanization was still mainly in the future in 1840, as was much of the impact of religious reform. Although less elitist in its interests than conventional historiography of German Jewry, this is not really a study of "popular culture." It deals with the ideology of communal leadership and not much with the life or ideas of the followers. One could also argue with its emphasis on voluntary organizations and its downplaying of the role of the official Jewish community (*Gemeinde*).

Despite these criticisms, this book represents an important and original analysis of a complex and significant transition in modern Jewish history.

STEVEN M. LOWENSTEIN
University of Judaism

W. E. MOSSE. *Jews in the German Economy: The German-Jewish Economic Elite, 1820–1935*. New York: Clarendon Press of Oxford University Press. 1987. Pp. 420. \$67.00.

Studies of economic elites may not be popular in this day of social history, but W. E. Mosse believes that Germany's Jewish economic elite deserves to be restored to a place of prominence. Denied it during the Weimar years by apologists intent on

contradicting anti-Semitic charges of Jewish domination, the Jewish economic elite was later neglected by a new generation of historians more interested in shopkeepers and cattle dealers than bankers, industrialists, and commercial magnates. Mosse's impressive documentation of top-level Jewish economic activities between the Restoration and the Third Reich shows that they were neither marginal nor short-lived. The Jews' share of large fortunes and of coveted titles (*Kommerzienrat* and *Geheimer Kommerzienrat*) granted for services to the economy and the larger community, nearly 20 percent of the totals, reflected the level of Jewish participation in the imperial German economy. Moreover, about one-third of Germany's corporate elite of managing directors and board chairmen was Jewish. There was no dramatic decline of Jewish economic importance in Weimar Germany, Mosse argues. Leading Jewish bankers and businessmen were no harder hit by inflation and depression than were their gentile counterparts. The real decline began only in 1935. Mosse acknowledges that titles, wealth, and positions cannot automatically be equated with influence, but he maintains that they are the only reliable indices. His argument is weakest for the Weimar years, for which he limits his evidence almost exclusively to positions and minimizes the effects of the worsening political climate.

Mosse's most stimulating passages explore the links between ethnicity and the achievements of his elite. Rejecting marginality theories, he argues that the Jews' minority status never restricted them to fields disdained by gentiles. On the contrary, from the beginning, many of them joined the main currents of German economic life. Mosse finds no evidence that they became marginal by cleaving to family firms in the age of high industrialization, differing in this instance with Arthur Prinz, *Juden im deutschen Wirtschaftsleben* (1984). Mosse places his Jewish elite among the moving figures in joint-stock banks (with their unique ties to German business and industry) and hence in the rationalization of all kinds of German enterprises. His structural analysis locates the origins and persistence of distinctive Jewish economic patterns in preemancipation concentrations that were reinforced by informal kinship and dynastic relations. The result, he concludes, was nothing less than the formation of a Jewish sector of the economy, never entirely sealed off from the gentile sector but always concentrated in credit and commercial activities, and not excluding industrial entrepreneurship. A few comparisons with gentile elites substantiate his conclusions about distinctive Jewish economic structures and patterns of internal migration. More tentatively, Mosse refers to the Jewish elite's receptiveness to international

trade, technological innovation, risk taking, and the new instruments of advertising, specialty shops, and department stores. Although opening the door to the possibility of a specifically Jewish economic "style," he cautiously shuns what he cannot easily quantify. This important aspect of the Jewish economic elite awaits the attention of more traditional social and cultural historians.

Students of German anti-Semitism will be struck by Mosse's finding that members of the German gentile economic elite rarely displayed racial prejudice toward Jewish colleagues and rivals. His Schumpeterian portrayal of German capitalists strengthens the view that the tragedy of German Jewry after 1918 arose in part from the political manipulation of popular resentments against highly visible Jewish elites. The decline of the Jewish middle classes meant little when nationalist and anticapitalist passions could be focused on a few hundred cosmopolitan Jewish bankers and entrepreneurs.

DONALD L. NIEWYK
Southern Methodist University

DIRK BLASIUS. *Ehescheidung in Deutschland 1794–1945: Scheidung und Scheidungsrecht in historischer Perspektive*. (Kritische Studien zur Geschichtswissenschaft, number 74.) Göttingen: Vandenhoeck und Ruprecht. 1987. Pp. 281. DM 54.

In his important studies of criminality in nineteenth-century Prussia, Dirk Blasius successfully negotiated the boundaries between legal and social history and revealed the points where the two intersected. This book continues that project, though with a very different subject matter, the reform of marriage and divorce law in Germany in the nineteenth and twentieth centuries. Blasius demonstrates convincingly how laws have shaped conceptions of marriage and the family and how, in turn, popular conceptions of gender relations, reflecting changes in people's lived experience, have brought about reforms of the law.

Blasius's account is not one of unimpeded "progress" or "emancipation" from the late eighteenth century to the twentieth. Indeed, the story of the nineteenth century is one of the steady deterioration of women's rights within marriage and of the decline in status of unwed mothers. The relatively liberal regulation of divorce under the Prussian Civil Code of 1794 (*Allgemeines Landrecht*) included the possibility of divorce by mutual consent on the grounds of "insurmountable antipathy" (*unüberwindliche Abneigung*). This provision came under attack from conservative political and clerical forces, which insisted on the church's formal right to intervene in divorce proceedings

in order to counsel reconciliation. Conservatives and clerics also attacked the provision of the Prussian laws that allowed unmarried mothers to make legal claims for child support from their children's fathers. Such provisions were condemned for inviting promiscuity and "free marriages" (*wilde Ehen*). Women should be held responsible for their actions, not allowed to shift the blame to the alleged fathers of their children.

The introduction of civil marriage (*Zivilehe*) in the *Kaiserreich* presented a direct threat to the church, but Blasius reminds us that clerical authority continued to have a significant influence on the social practice of many Germans. Moreover, the culmination of the nineteenth-century reform of marriage and family law in the civil code of 1896 (*Bürgerliches Gesetzbuch*) represented not emancipatory tendencies but rather a "tradition in legal practice of blocking legal progress" (p. 81). This was particularly evident in the code's emphasis on legal marriage as normative and in its insistence that all adult sexual relations outside of marriage be classified as deviant. The code also made divorce much more difficult. Legally defined marriage and the family were crucial parts of the bourgeois state's conception of social order and morality.

Social Democratic and left liberal reform attempts in Weimar proved unsuccessful. Although the Center was consistently a supporter of reform in many areas, it rejected any proposals for the liberalization of divorce law; confessionally defined ideological concerns created a major fissure within the fragile coalition that carried Weimar democracy. Rather, it was the Nazis who greatly eased restrictions on divorce. In 1938 they diluted the provisions of the civil code and even established a three-year separation and claims of irreconcilable differences as grounds for ending a marriage. The Nazis aimed their policies at advancing a racist and pronatalist ideology; unhappy marriages would not produce the next generation, and "Aryans" should be permitted to separate easily from Jewish spouses. But Blasius also insists that, for the majority of the population, the 1938 provisions offered "the opportunity to shape one's living space [*Lebensraum*] more autonomously and on a legally more secure basis" (p. 189). For him the 1938 law thus becomes another complicated issue in evaluating the modernity of the Nazi state.

Blasius is at his best when he is describing ministerial and political battles surrounding proposals for divorce reform. He also offers much statistical material and occasional vignettes from divorce proceedings, which provide a sense of the relationship between legal theory and social practice. He rightly emphasizes that his story is a

central part of nineteenth- and twentieth-century German women's history, but his attempt to locate his account in this context is less successful. Although he indicates the gender-specific implications of marriage and divorce reform, he does not explore how these issues became important concerns of bourgeois and socialist women's organizations in both the *Kaiserreich* and Weimar. He also does little to relate the legal reform he describes to larger ideological issues such as the "New Woman," the rise of nationalist pronatalism, or social concerns about working mothers and the declining birthrate. Weimar reform proposals become part of larger party political struggles, not attempts to answer the "Woman Question." Still, there is no doubt that those who wish to pursue these larger dimensions will find this book an informative starting point. It fully accomplishes the author's objective of establishing the importance of legal, institutional, and political change for understanding the history of the modern family.

ROBERT G. MOELLER
University of California,
Irvine

JOHN C. G. RÖHL. *Kaiser, Hof und Staat: Wilhelm II und die deutsche Politik*. Munich: C. H. Beck. 1987. Pp. 262. DM 39.80.

The author of this collection of previously published and here slightly revised essays is well known as the editor of several volumes of valuable documents. Most recently, he published Philipp Eulenburg's political correspondence, and he is now writing a definitive biography of Emperor William II, who came to the German imperial throne at the age of twenty-seven, one hundred years ago. He left it in disgrace in 1918 and died at Doorn, in Holland, in 1941. In the English-speaking world, the last two decades have witnessed the publication of three biographies of Emperor William, a major study of the imperial entourage (by Isabel Hull), and an important collection of essays edited by John C. G. Röhl himself (*Kaiser Wilhelm II: New Interpretations* [1982]), but the German people have been less well served by up-to-date studies of their last, and arguably their most flamboyant, emperor. This gap is the *raison d'être* of these essays—on the kaiser's personality, on his friend, Eulenburg, on court society and William's much remarked "personal rule" (following Norbert Elias, Röhl prefers the term "kingship mechanism"), and on higher officialdom, the diplomatic service, and the military in pre-1914 Germany.

Methodologically, Röhl is an unrepentant

"Kehrite," like Fritz Fischer, Volker Berghahn, and one or two others. Indeed, he sets out to show that in the German empire the person of the all-highest, as William II liked to think of himself, counted for much more than is admitted by the structuralist champions of the "new orthodoxy" of Bielefeld provenance (above all, Hans-Ulrich Wehler) or by the paladins of the still newer would-be orthodoxy represented by the current generation of people's historians, such as Richard J. Evans and Geoff Eley. Basing his case on meticulous and extensive archival research, as well as a thorough grasp of the prodigious secondary literature in several languages, Röhl is unimpressed by the "view from below" and concentrates instead on the old elites at the helm of imperial Germany in the Wilhelmine era.

In so doing, he invites some obvious criticisms. For example his somewhat narrow focus on the person of the kaiser is now more than unfashionable. We do need to know more about the influence of other key personalities, such as Theobald von Bethmann Hollweg, the last peacetime imperial chancellor. We also need to be told why, if William II was really such a crucial and autocratic figure, the military were able to brush him aside so effortlessly after August 1914. In fact we need to know a lot more than we are told here about the influence of parliament, the political parties, and the interest groups in the *Kaiserreich*, and also about the wider sociocultural context, including the mentalities of the Wilhelmine age. Possibly least satisfactory to modern German historians is the author's insistence on remaining as belletristic as possible. One might overlook his fondness for trivia, as when he expatiates on the size and cost of the royal establishment, but few readers, in Germany or elsewhere, are likely to be much impressed by his final chapter, for instance, where he deals with the influence of the military and the making of World War I. This catastrophe is reduced, once again, to a virtual conspiracy of the kaiser and his generals, hinging on the so-called war council of December 8, 1912, albeit with the disclaimer that Röhl himself professes to have "never drawn a direct line" from 1912 to August 1914 (p. 251 n. 113). Since even Fischer seems now to have abandoned this extreme position, Röhl here appears capricious and adrift.

Yet Röhl's first essay, on the kaiser's personality, is scarcely less unconvincing. Much emphasis is attached to the minimal brain damage possibly inflicted on William at birth, causing a condition now known as attention deficit disorder, or ADD. Although this might help account for some of William's behavioral problems as a child, available medical research indicates that most ADD sufferers by the time they reach adulthood have lost

their "soft" signs (such as hyperactivity, impulsiveness, inattentive and sometimes violent behavior) and generally behave in ways that are thoroughly "normal." Equally obvious question marks hang over Röhl's overinterpretation of the psychological damage allegedly inflicted on young William by an unloving English mother.

On the other hand, perhaps it is good that we are given this reminder that history, especially that of a semiabsolutist military monarchy such as imperial Germany, which was, after all, a far cry from liberal England or republican France, is not made exclusively by impersonal structures or determined wholly by mechanistic processes. By demonstrating that Cleopatra's nose, or an emperor's flawed personality, can have decisive effects on the destinies of millions for generations, Röhl provides a valuable complement to the Bielefeld account of "Wilhelminism without William" that is surely no more fanciful or far-fetched than, say, an attempt to rewrite nineteenth-century German political history (or at least that of the German empire's second largest city) from the standpoint of the 1892 cholera epidemic in Hamburg. Though less than astoundingly new in its method or findings, Röhl's collection of essays is a lucid, literate, and legitimate addition to the available literature on the political history of the German empire. His forthcoming biography will, however, need to pay more careful attention to the wider context if it is to appeal to scholars while also satisfying the demands of the nostalgia boom in the Federal Republic under present Christian Democratic management.

ROGER FLETCHER

Harpenden, United Kingdom

PAUL LETOURNEAU. *Walther Rathenau ou le rêve prométhéen: Pensée politique et économique (1867-1922)*. (Thèses ou Recherches, number 26.) Quebec: Naaman. 1987. Pp. 367. \$30.00.

While threatening to hollow out its *raison d'être*, Paul Létourneau has achieved a thoroughly useful book. Examining Walther Rathenau as a political and economic thinker, Létourneau finds Rathenau's thought shallow and pretentious, altruistic in its claims but patently self-serving, promiscuously derivative, elitist and racist in the Social Darwinian manner, fascistic, and, expressing a divided nature, contradictory from beginning to end. Indeed, of themselves Rathenau's ideas were not merely worthless but pernicious, encouraging magical thinking and working the short-term seduction of unprotected minds.

Yet the study of a bad thinker can be productive. Rathenau's thought was not all bad, and its

few excellences were important. One can be grateful to Létourneau, who, dispensing his readers from doing the same, has searched through the dross and separated out those excellences. Rathenau, guided by his better ideas, moreover, ably served a good cause during the Weimar Republic's years of early sorrow.

Developing his public thinking around his private problems, Rathenau, as Létourneau sees him, "tormented himself about his [Jewish] origins to the point of making a sickly cult for himself" (p. 49). Although trained as an engineer, Rathenau retained no trace of science in his thought, believing it incapable of measuring values (p. 84). Abusing the word "law" to concoct statements showing no stable relations, he attempted to generalize the banal products of his all-determining intuition and too often expressed little more than the command to humanity to be good (p. 154).

Létourneau, keeping his balance, refuses either to permit familiarity to condemn or partiality to excuse too much. His subject, besides a number of articles, wrote ten books or pamphlets, most of them best sellers. *Von kommenden Dingen* (1917), which sold seventy-two thousand copies, was both the best and the most baneful of Rathenau's writings. As Létourneau notes, its subordination of the individual to the collective made it totalitarian in its ultimate implications. Yet it blundered into the opening between rigid socialism and helplessly laissez-faire capitalism that a responsible thinker such as John Maynard Keynes could more clearly see.

In May 1921 Rathenau resigned as figurehead board chairman of Germany's second-largest electrical company (which his father had founded) to move with smooth inevitability into the nation's cabinet. There he demonstrated a sense of reality (again to be credited to Emil Rathenau) that had always subsisted under the sham. Walther Rathenau was charged with the responsibility for negotiating reparations payments, the central factor in German foreign relations at the time. In January 1922 he became foreign minister, signing the Treaty of Rapallo on Easter Sunday and, with this initiative, profoundly altering the reparations issue and the situation of Germany. His assassination on June 24 made his contribution to Germany and the world perfect and, when one thinks of the concurrent rise of Nazism, perversely fateful. Létourneau might have made the connection between thinker and statesman more explicit.

DAVID FELIX

*Bronx Community College and Graduate Center
City University of New York*

FRANZ J. BAUER, editor. *Die Regierung Eisner 1918/19: Ministerratsprotokolle und Dokumente*. Assisted by DIET-

ER ALBRECHT. (Quellen zur Geschichte des Parlamentarismus und der politischen Parteien, first series, Von der konstitutionellen Monarchie zur parlamentarischen Republik, number 10.) Düsseldorf: Droste. 1987. Pp. cv, 486. DM 118.

HEINRICH POTTHOFF and HERMANN WEBER, compilers. *Die SPD-Fraktion in der Nationalversammlung 1919–1920*. Foreword by HEINRICH POTTHOFF. (Quellen zur Geschichte des Parlamentarismus und der politischen Parteien, third series, Die Weimarer Republik, number 7.) Düsseldorf: Droste. 1986. Pp. lxviii, 257. DM 78.

We have here two more carefully edited, handsomely produced documentary collections from the Commission on the History of Parliamentarism and the Political Parties in Bonn. The more substantial of the two is the volume on Bavaria's socialist government under Kurt Eisner during the revolution of 1918–19. This work is somewhat similar in format and purpose to the classic two-volume edition of the records of the Council of People's Commissioners in Berlin during the same months (published in the same series in 1969). The present collection gives all surviving records of actual cabinet meetings supplemented by the texts of major government decisions and manifestoes plus a few other immediately relevant documents, all annotated with reference to government files and private papers as well as the contemporary press, memoir literature, and scholarly commentary. There is a substantial scholarly preface. It is all admirably done.

It must be noted, however, that the work published in 1969 had advantages over the present one. The earlier edition's central documents, the cabinet minutes, had then been available to scholars for only about a decade; they presented awkward gaps and interpretative problems and had not been entirely assimilated into the scholarly literature. That publication was therefore in a position to open up new perspectives; the long introduction to those volumes by Erich Matthias amounted to a small monograph that remains a starting point for understanding not only the government in the narrow sense but also its place in the wider events of the day. Franz J. Bauer's edition, by contrast, presents documents that have been undergoing evaluation by scholars for over twenty-five years: works by Fritz Schade (1961) and Allan Mitchell (1965) long ago laid the groundwork for a good deal of subsequent research. Bauer has unearthed a few significant new details (such as that the leaders of the Social Democratic party [SPD], not Eisner, probably took the initiative in forming a socialist coalition government), and he has laid a secure foundation for

much that is less new. But it is not pathbreaking work in any sense.

The second volume under review, compiled by Heinrich Potthoff and Hermann Weber, presents the minutes of the SPD's National Assembly delegation between February 1919 and February 1920 (supplemented by a single document from another source that records a session of March 19, 1920). These records, which likewise have been open to scholars for many years, are mostly terse and dry. Furthermore, they deal with such a short time span that it is hard to extract from them trends of wider significance—unlike their predecessor collection, the two-volume edition of SPD delegation records that covered the period between 1898 and 1918 (published in 1966 in the same series). While Matthias was able to write another of his classic prefaces for the earlier publication, Potthoff confines his introduction to more technical matters and furthermore tends to look back for comparisons to the period before 1918 rather than forward into the 1920s, for which all comparable records have been lost. All in all it seems that this volume, though by no means without interest, was added to the series more because of a promise made years ago than out of a compelling need to have these documents in print.

Neither volume can be said to break new ground, but both will be solidly useful to scholars and graduate students who can now consult in their own libraries authenticated, annotated texts that formerly had to be sought out in Munich or Amsterdam.

DAVID W. MORGAN
Wesleyan University

WALTER F. PETERSON. *The Berlin Liberal Press in Exile: A History of the Pariser Tageblatt-Pariser Tageszeitung, 1933-1940*. (Studien und Texte zur Sozialgeschichte der Literatur, number 18.) Tübingen: Max Niemeyer. 1987. Pp. xv, 287. \$65.00.

What he loved about Paris, said that perennial exile James Joyce, was its "racecourse tension." For their experience of the city of light the German exiles from Adolf Hitler might have used a similar description. But, while for Joyce the racecourse analogy was meant to be positive, evoking the vital aspects of competition and risk, for the German émigrés it would have been essentially ironic, hinting at the dark side of the sport of kings: the forced conviviality, the air of intrigue, the atmosphere of earnest gambling. Walter F. Peterson, in a book that is the result of a very thorough research effort, looks at the Parisian experience of one significant group of émigrés from the Third Reich, the coterie of journalists

and intellectuals around the *Pariser Tageblatt* (after 1936 *Pariser Tageszeitung*). About the somber lives of these left-leaning liberal idealists, who in their writing projected bright visions of tolerance and humanity, he tells a sad tale. It is one involving frustration, hate, and fear, among other, for the most part, equally unattractive human attributes. Magnanimity did not flourish in exile. And the caged, wounded, and bloodied egos of intellectual politicians do not a pleasant subject make.

Peterson apparently set out initially to write a biography of Georg Bernhard, the erstwhile editor of the *Vossische Zeitung*, Reichstag deputy, Jewish activist, and after 1933 editor of the *Pariser Tageblatt-Tageszeitung*, whose career spanned three epochs of German history. For reasons he does not make clear in his prefatory remarks, Peterson ended up concentrating on the last of the three periods and producing, instead of a biography of a dynamic and tormented man, a more diffuse account of a little dynamism and a great deal of torment in the German emigration.

The conundrums involved in publishing an, above all, anti-Nazi as opposed to proliberal newspaper in exile are set out well in this study. Despite his competent analysis of editorial policy, however, Peterson is in the end most keenly interested in, and he has gone to great lengths to try to document, the crepuscular cloak-and-dagger aspects of life in exile: the kidnappings, the terror, the spying, and, particularly, the internecine squabbling among the émigrés. Much of his effort is devoted to an attempt to unravel the so-called *Pariser Tageblatt* affair, which erupted suddenly in June 1936 in the wake of a printed charge by the editorial staff of the *Tageblatt* that Wladimir Poliakov, the owner, had attempted to sell his newspaper to agents of the Third Reich.

Peterson has not managed to unravel all the threads in this Parisian quilt, but he has produced a readable, at times even enthralling account, which will find its niche in the bibliography on the German press and on the German emigration.

MODRIS EKSTEINS
University of Toronto,
Scarborough Campus

PETER HAYES. *Industry and Ideology: IG Farben in the Nazi Era*. New York: Cambridge University Press. 1987. Pp. xxviii, 411. \$39.50.

The lack of detailed and intelligent studies of large firms in the Nazi period has long been a just reproach to historians. To write one means to face up to the many problems of public and private morality that are involved, as well as to unravel the complex connections between firm and govern-

ment. In choosing to write about the most famously wicked of them all, the chemicals trust I. G. Farben, twenty of whose directors were tried in 1947 as war criminals, Peter Hayes shirks nothing. Earlier histories, meretricious and politically disputatious, were more a hindrance than a help, and the reader of this volume should first read the footnotes to see from what a maze of sources the author has worked and how much of his life he has surely had to give to it. The time and energy were not wasted, for he has written an excellent book, the best work that exists on business under the Nazis.

I. G. Farben was a large multinational imperfectly controlling a collection of enterprises producing a wide variety of products for different markets and often working on the frontiers of technology to do so. The nature of the firm inevitably meant that the balance sheet of its relationship with the Nazi regime was difficult to draw up, even for its directors. Its large, misplaced investments in the previous decade in synthetic fuel production were safeguarded under the Nazi government by high prices, guaranteed markets, and tariffs, but this was a poisoned chalice. The firm benefited from the curbing of labor unions, although it had by no means been among the agitators for such a policy before the Nazis came to power, whereas it lost through the intrusion of the Nazi Labor Front into its plant operations. Rearmament was instrumental to making new areas of research and development financially successful. Coupled with policies of increased agricultural protection, for example, it eventually justified the firm's massive investments in nitrogen technology. The import substitution policies and anti-Semitism of the government, however, meant that, except in Southeastern Europe where it maintained its 1928 export level and bought back its debts, I. G. Farben lost on foreign markets part of what it gained in the domestic market. It was forced to cough up large political contributions, about 40 million Reichsmarks all told. Overall, its profits increased and did so in accelerating fashion, although, because of the terms on which its new undertakings were jointly financed by the government, the dividends that it paid to shareholders did not do so. Between the start of 1939 and the end of 1944, its retained earnings were twice the total of the preceding thirteen years. More than twice the value of these retained earnings had been invested in new plant, about 30 percent with government aid, and fifty-nine new war factories that I. G. Farben leased and operated were available for easy acquisition. On the eve of nemesis, therefore, the purely financial aspects of the balance sheet were positive.

The nonfinancial aspects of the balance had by

that date, though, become the more important part of the total. In effect the firm's investment path had been chosen by the government. It had conceded to heavy government pressure to cash in on short-term profit chances by producing synthetic *Zellwolle* derivatives rather than pursuing the longer-term prospect of polyvinylchloride products, which it had originally preferred. The decision to build a large rather than a small synthetic rubber (buna) plant at Schkopau was taken on the same grounds. Faced with a capricious, authoritarian government, it opted for a strategy of making itself indispensable. This led only to overexpansion, especially in new synthetics, which took one-third of its new capital expenditure although the demand for them was almost entirely military. It was allowed to establish fixed assets with its own income and to recover the cost over a decade through high prices, profits, and accelerated depreciation. But what then? We can stand amazed at the capacity of a large private company to keep business secrets even from so ferocious a government even in a patriotic war, but the firm had become nevertheless the prisoner of a competitive system whose rules it was far from making. In this competition it blackened its name forever after conceding to pressure to erect a new plant at Oświęcim (Auschwitz) and by its pitiless treatment of the concentration camp labor it used there. On average thirty-two workers died per day. If most of the firm's active directors showed little or no overt political sympathy for the Nazi regime, there is no evidence that a single one of them wanted to act against these atrocities. "When political changes condition profit making in ways that tend to have immoral results, a capitalist system," Hayes concludes, "will prove strikingly malleable" (p. 381). More than any other firm, I. G. Farben was admired as the epitome of professional management by highly qualified experts. Its history bears out the author's suggestion that one of the attractions of professionalism is the opportunity to avoid moral decisions.

It seemed impossible any longer to write an important work about the Nazi regime, but Hayes has demonstrated that this is not so and has done so stylishly.

ALAN S. MILWARD

London School of Economics and Political Science

WILLI A. BOELCKE. *Die Kosten von Hitlers Krieg: Kriegsfinanzierung und finanzielles Kriegserbe in Deutschland 1933–1948*. (Sammlung Schöningh zur Geschichte und Gegenwart.) Paderborn: Ferdinand Schöningh. 1985. Pp. 220.

This is a book with a moral: an economy gets the currency it deserves. The author's subject is the

"first dammed up paper money inflation in history," something "that Hitler ordered and executed and that was maintained by Allied command for another three years after [his] end" (p. 7). The final and most important of Willi A. Boelcke's planned trilogy on the economy of National Socialist Germany, this book describes simply, accurately, and thoroughly the process by which the Ministry of Finance, the Reichsbank, and other central government agencies appropriated both the private savings of the nation and the monetary resources of the occupied countries in order to finance rearmament and war. It also adds significantly to knowledge about the still largely obscure prehistory of the currency reform of 1948.

Unlike previous descriptions of the national finances of the Third Reich, which are either apologetic in purpose or technical in approach, Boelcke's account does not end in 1945. He maintains that the hidden postwar inflation was the inevitable consequence of Hitler's policies, the link between the two eras being the decision of the occupation powers to retain the National Socialist system of wage and price controls. This bold thesis is a bit overdrawn but by no means undermines the value of the book.

Boelcke's contention that Germany in effect got what it deserved is morally reassuring but historically unconvincing. Until defeat was imminent, the exploitative system of Nazi war financing worked well, as Boelcke admits. Workers and farmers toiled with little complaint for low and only partially disposable wages and payments. While useless earnings flowed into effectively immobilized savings accounts, banks and other financial institutions pumped money into the armaments economy in return for government paper impossible to redeem at par. Thus, veiled confiscation was the operative principle.

The directors of German finance and industry understood that the currency was debauched and that some combination of repudiation and revaluation would be necessary regardless of the war's outcome. They resorted to barter and hoarding, which, as Boelcke and others have shown, became institutionalized as methods of doing business. Yet a flight from the currency never occurred; until the very end, black markets were all but absent in Nazi Germany. It was only in occupied Europe, where authority broke down, that inflation disrupted public order and, in the case of Greece, unleashed social and political chaos.

Contrary to what Boelcke says, hidden inflation after 1945 was not the inevitable consequence of the Third Reich's financial policies. The peculiar postwar monetary situation in which currency could be used to make purchases at official prices but had no other transactional value whatsoever

was due to material destruction and economic dislocation, on the one hand, and a paralysis of policy, on the other. The Allies could have introduced a new German currency at any time after 1945. What prevented them from doing so, as Boelcke's own evidence reveals, were two considerations extraneous to the state of Reich finances. One of them was gridlock at the Allied Control Council; the other, the general fear that production and transportation bottlenecks would skew prices radically, make industry uncompetitive, and inflict unacceptable hardship on the public.

One might assert that the author has done little to clear up the central mysteries of Nazi monetary and credit policies, fault him for failing to provide new explanations as to the workings of the novel and curious financing devices adopted by the Reich in the 1930s, and cast doubt on his conviction that the "hidden paper money inflation" of Hitler's Germany was without significant historical precedent.

One could add that Nazi financing was not as consequential as he believes it to have been. Undeniably, the savings of the vast majority of Germans not fortunate enough to have owned stock were wiped out. Still, the Third Reich's credit methods did not impede the expansion of armaments production, were not the cause of occupied Germany's later economic miseries, and did not prevent the Federal Republic from becoming prosperous and stable. Hitler may in fact have done no more serious damage to German national finances than to those of either the occupied or victor nations.

Even with its faults, this book is a major breakthrough. It shifts the locus of discussion of National Socialist finances from methods and techniques of credit creation to the system of economic and political control that made it possible for them to work effectively. It also establishes a new context for future studies of the currency reform of 1948. Boelcke shows that its success presupposed a basic restoration of economic order, that its execution was often ragged, that political decisions (in particular regarding "the equalization of burdens") partly determined how it worked, and that it did not eliminate the costs of the war at a single stroke. A settlement of the prewar debt was arrived at only in 1953, and convertibility did not occur until 1958. Thanks to this major work, it is no longer possible to present the financial history of the Third Reich as if it ended in 1945; Hitler's curse hung over German credit and payments for nearly half a generation.

JOHN GILLINGHAM
*University of Missouri,
St. Louis*

ROBERT JAY LIFTON. *The Nazi Doctors: Medical Killing and the Psychology of Genocide*. New York: Basic Books. 1986. Pp. xiii, 561. \$19.95.

Psychiatrist Robert Jay Lifton's *The Nazi Doctors* is an impressive and impassioned book in the tradition of such earlier works of his as *Death in Life* (1968), on survivors of Hiroshima, and *Home from the War* (1973), on Vietnam veterans. These studies are based largely on interviews, a method, of course, fundamental to the psychiatric profession, and, in the present book, on the heavy use of secondary works in German history. Although Lifton credits a number of archives in Europe, Israel, and the United States, his book does not represent an interpretation based on documentary findings on the subject of the Holocaust, as do such works as Gerald Fleming's *Hitler und die Endlösung* (1982) or Christopher Browning's *The Final Solution and the German Foreign Office* (1978).

Lifton instead wishes to advance his notion of the metapsychological "paradigm of death and the continuity of life" (p. 467) inspired by the work of Freud disciple Otto Rank. According to Lifton, "human beings kill in order to assert their own life power" (p. 467). This idea serves as the basis for what Lifton calls "doubling" or "the division of the self into two functioning wholes" (p. 418), a process that allowed SS doctors to cure by killing in the hellish environment of the concentration camps. To be sure, Lifton argues, the eugenic trend within biology and medicine and certain German cultural traits such as "the perennially 'torn condition' of the German self" (p. 468) combined to provide a collective historical dimension to the response of doctors to "the other planet" that was Auschwitz. The creation of an "Auschwitz self" was part of the realization of a Nazi "biomedical vision" that was the "central psychohistorical principle of the regime" (p. 4).

Lifton is right in stressing the equation of what Rudolf Hess called "applied biology" with National Socialism, and his interviews with former Nazi doctors constitute a valuable resource. But there are limitations to his approach (see the symposium in *The Psychohistory Review*, Fall 1987). Lifton interviewed twenty-eight doctors, five of whom had worked in the camps, twelve nonmedical professionals, and eighty former prisoners at Auschwitz who served in the medical blocks. We also learn something through these interviews about the Nazi medical higher-ups who fled, committed suicide, or were executed after the war. But is this a sufficient data base on which to build an ambitious characterization not only of Nazi doctors but of German history and the human "symbolization of life and death" in general (p. 467)?

Lifton's concept of "doubling" is also proble-

matic in this regard, for it too easily misses the postwar dissembling one would expect from these SS doctors (see, for example, pages 161, 444-45). Did Nazi doctors "double" or simply act out of a tangled consistency of motives? Lifton's "doubling" is a mechanism designed to explain the human capacity for performing evil deeds with a clear conscience. But, whatever the virtues of this concept in offering a dynamic alternative to behavioralism for the investigation of human interaction with the environment, it remains a relatively static, synchronic, and descriptive concept when compared to the historian's diachronic search after conditions and motives. For Lifton Nazi doctors (and the German nation as a whole) were consumed with "death anxiety" (p. 448), hence the racist fear of "contamination." But a more Freudian (and historical) approach would emphasize the content of such irrational hatred and fear based on specific personal and historical experiences, thus, among other things, abandoning Lifton's outdated generalization about the German psyche. We need to know more about the individual and professional pasts of doctors in general in Nazi Germany in order to determine predispositions to various degrees of collaboration in Nazi designs and atrocities. Michael Kater's forthcoming study of the medical profession under Nazism will surely go some way toward filling this gap. In sum Lifton's limitations as a historian are a function of his admirable and effective concern as an activist psychiatrist with the processes that both endanger and ennoble humankind. Lifton writes in prescription for the present and the future. The historian necessarily must spend more time with the past.

GEOFFREY COCKS
Albion College

RICHARD MACKENNEY. *Tradesmen and Traders: The World of the Guilds in Venice and Europe, c. 1250-c. 1650*. Totowa, N.J.: Barnes and Noble. Pp. xvi, 289. \$29.95.

Craft guildsmen represent a definable group in a Renaissance town, according to Richard Mackenney. He discovers that the adaptability of Venetian guild members contributed greatly to that city's social stability and economic strength. In addition he compares the guilds of Venice with other early modern European guilds. The book's strength lies in the evidence from the fifteenth and sixteenth centuries collected in the last four chapters. Chapter 3 discusses Venetian craft innovations, particularly within the silk, apothecary, and mercers guilds. Chapter 4 demonstrates that advertising and selling local products during Venice's numer-

ous festivals provided a commercial excuse for the multiplication of these feast days. Chapter 5 attempts to explain how the Inquisition discouraged Venetian guildsmen from contacts with foreign merchants and from the commercial innovation that had characterized the guilds earlier. Chapter 6 discloses that, when the guilds protested, the government reduced the quotas of oarsmen drafted from the guilds and also reduced its imposts on the guilds. Mackenney concludes that the guilds of Venice influenced public policy and provided an outlet for the creative energy of Venice's heterogeneous craftsmen and traders.

The first two chapters rapidly outline the organization, religious orientation, and social services of Venetian guilds before the Renaissance. Here, Mackenney profits from the work of other scholars, particularly Giovanni Monticolo, Brian Pullan, and Frederic C. Lane. The book bristles with good footnote references, 728 in all, revealing Mackenney's scrupulous acknowledgment of the work of others and careful reference to archival records. It presents a useful map of guild centers and parishes in early modern Venice (appendix 1) and an excellent twenty-page bibliography of primary sources and secondary works.

At the close of each chapter, Mackenney suggestively compares the Venetian experience with guilds in other cities. For example Venetian festivals are compared with guilds and festivals in Florence and in London. He compares the Inquisition in Venice with the French Inquisition and with Rome, Geneva, and London. The enterprise of Venetian guilds in preserving tradition or fostering innovation is compared with guilds in other sixteenth-century European cities. His comparisons of the charitable and devotional activities of Venetian guilds and religious confraternities with those of Florence, Nuremberg, and London, however, are not clear. He uses twentieth-century scholarship on non-Venetian guilds; he did no primary research in other cities. Mackenney proposes that Venetian guilds did not atrophy after 1500 but rather adapted themselves to new conditions, strengthening the state.

This book would have profited from more careful editing. For example there are numerous typographical errors and inconsistent rendering of Venetian place names and of the titles awarded to other authors. The chapter conclusions should have been rewritten. The author presents the evidence forcefully but suggests conclusions hesitantly and imprecisely, sometimes in a dependent clause or phrase, sometimes not until the opening paragraphs of the next section. The strength of this research can be understood only after rereading the text and studying chapter and section headings.

This book presents new material on Venetian guilds and comparisons with foreign guilds. It should stimulate further studies, but Mackenney has not yet exhausted the subject.

LOUISE BUENGER ROBBERT
University of Missouri,
St. Louis

GUIDO RUGGIERO. *The Boundaries of Eros: Sex Crime and Sexuality in Renaissance Venice*. (Studies in the History of Sexuality.) New York: Oxford University Press. 1985. Pp. viii, 223.

Venetian social history, relegated to subordinate status by the political and institutional orientation of the city's historiography, has not moved much beyond the work of Pompeo Molmenti a century ago. Guido Ruggiero's analysis of Venice's rich judicial archives, tracing prosecution for sex crimes as an indicator of official attitudes toward sexuality, is a pioneering and welcome addition to the literature.

The author supplies rich and ample evidence for his main observations: the class bias of prosecution, noble arrogance, widespread victimization of women, and the laxity of noble convents. He is persuasive in tracing the way in which the Venetian judiciary balanced legislative ideals and social reality. The claims of God, the state, family honor, and property had to be maintained, but inevitable sexual tensions suggested a pragmatic and flexible policy. Thus, fornication and adultery were often lightly punished because they provided outlets for passion and created quasi-familial relationships. Fornication and even rape were often regarded as a substitute for courtship, with the offender allowed to marry the victim and escape punishment. Only sodomy, desperately feared as offensive to God and subversive to social stability, prompted concerted attempts at eradication.

Moving from specific infractions to general conclusions is always tricky. Ruggiero has made his task harder by declining to move much beyond judicial data. Privileging criminal records over "intellectualized literature or philosophy" (p. 5), he bypasses texts such as sermons, confessors' manuals, humanist treatises, or the works of moralists. As a result his perception of official ideology comes almost exclusively from judicial sentences and preambles to laws, partial sources at best. His choice not to use "modern models of social psychology or anthropology" (p. 15) eliminates potentially valuable interpretive tools. The book aspires, for example, to reveal a distinct culture of illicit sexuality, but without a working definition of culture the point cannot be proven. Certainly, sex crime abounded, but that fact does not prove a

subculture with a self-conscious, coherent membership and common values and patterns of behavior.

Declining to look outside judicial records for support of broader statements, the author is thrown onto his own devices. Some explanations draw from cliché: the Clytemnestra complex (p. 154, n. 16), the "humanization of the world picture," and the "ontology of the Great Chain of Being" (p. 70). Some will raise eyebrows among specialists. Forfeiture of an adulterous wife's dowry was a lenient punishment (p. 51)? In fact a woman without a dowry lost status and was terribly vulnerable in the likely event of widowhood. Assignment of the dowry to the husband began only in the 1360s (p. 53)? How does the Roman law's *patria potestas* apply to a society that had repudiated Roman law (p. 54)? An upper-class woman was only a "sexual piece of property" (p. 154)? The author connects increasing levels of homosexuality with the arrival of humanistic studies from Florence, on the basis of four cases involving such uneducated Florentines as a barber and a tavern employee (p. 137). Frequently, the author gives up verification altogether and speculates freely. On page 153, for example, the reader is faced with "almost surely," "I wonder if," "might argue," "may have felt," "appear to have," "appears to have," "suggesting that," "seems to have," and "appears that." Finally, the book's conclusions exhibit the myopia caused by intense scrutiny of a single source. The Council of Ten was indeed increasingly active in fighting sodomy, but it was increasingly active in all sectors of public life, and fear of sodomy hardly took "center stage" in its deliberations. There is no evidence that the Ten "feared that a homosexual subculture threatened to become the culture of Renaissance Venice" (p. 140).

That Venetian women sired children is an amusing slip (p. 164), but the book's several errors of grammar and syntax are disruptive. It is unfortunate that a promising subject, solid archival work, and sensitivity to broad movements have been undercut by uneven presentation.

JAMES S. GRUBB
University of Maryland,
Baltimore County

FRANCO VENTURI. *Settecento riformatore*. Volume 5, *L'Italia dei lumi (1764–1790)*. Part 1, *La rivoluzione di Corsica; Le grandi carestie degli anni sessanta; La Lombardia delle riforme*. (Biblioteca di cultura storica, part 1, volume 5, number 103.) Turin: Giulio Einaudi. 1987. Pp. xiii, 857. L. 85,000.

With the fifth volume of his series *Settecento riformatore*, Franco Venturi, one of Europe's foremost

scholars of the Enlightenment, returns to the Italian focus of his first volume, *Da Muratori a Beccaria*, published in 1969. As always in Venturi's work, however, the events and people of eighteenth-century Italy are placed in a larger context. Regional and local history serves a dual purpose. It contributes fascinating details, colorful characters, and richness of texture to the recounting of the past. At the same time it allows Venturi to illustrate those general trends in eighteenth-century Western society that transcended local or regional traditions.

The subtitle of this volume, *L'Italia dei lumi (1764–1790)*, summarizes Venturi's current program of work: to complete a trilogy on the Italian states in the decades of the 1760s, 1770s, and 1780s, choosing for each decade the episodes that most vividly illustrate the advances and the limitations of the reforming impulse of the Italian Enlightenment.

The trilogy (two more volumes are in preparation) opens with the description and analysis of three major Italian developments of the 1760s: the Corsican rebellion against Genoa, the consequences of famine in Naples, the Papal States, and Tuscany, and the impact of political and economic reform theories in Lombardy.

Venturi's methodology blends historical narrative, sometimes minutely detailed and copiously footnoted, with bold generalizations, learned, sophisticated comparisons, and subtle analysis. All this explains the massive size and scope of this work, which requires substantial knowledge of Italian and European events and much persistence on the part of the reader.

An intellectual feast awaits those willing to savor the book slowly, a few pages at a time. Although the narrative occasionally becomes repetitive and the footnotes too long, there is much new information on the 1760s (the fruit of diligent archival research), and there are unforgettable vignettes of popular culture (for example, songs and poems from famine-stricken Naples) and portraits of important Italian leaders of the period.

Each part of this volume could stand alone as a significant contribution to the monographic literature on the 1760s. By treating them as three long chapters of the same volume, Venturi implicitly calls to our attention the most significant Italian developments of that decade. One of these, perhaps the most uniquely Italian, was the irreversible decline of the patrician (or aristocratic) republics, whose power had depended on control of maritime routes and domination of the surrounding countryside. Venturi's analysis of the Corsican rebellion against Genoa illuminates the reasons for that decline even while explaining why the Corsicans, whose cause and whose leader enjoyed

widespread support in Europe and North America, could not hold on to their hard-won freedom.

Another dramatic development of the 1760s was the crisis of food production and distribution that afflicted thousands of victims in southern and central Italy. Venturi uses three case studies, Naples, the Papal States, and Tuscany, to understand the causes of famine, the short-term effects on demographic patterns, and the long-term effects on policy.

The material lends itself to colorful descriptions of social unrest caused by famine and of various governmental and ecclesiastical attempts to deal with unrest. But Venturi's most important contribution consists of his comparative analysis of lessons drawn by political leaders and economists from the tragedy of the 1760s.

The third chapter covers issues and men more familiar to students of eighteenth-century history and literature. Venturi's treatment of Lombardy in the 1760s does not offer new interpretations of government policy or important additions to our understanding of such well-known reformers as Carlo Firmian, Gian Rinaldo Carli, or Pietro Verri. It does, however, make a convincing case that Lombardy, wealthy, stable, and open to ideas from several countries and cultures, was the first region of Italy in which general theories about economic reform and about the role of the state in that and other reforms could be developed and tested. The primacy of the Lombard reform movement, Venturi argues, did not consist in the originality of its thinkers, more impressive in Naples, or in the openness of its rulers, more evident in Tuscany, but in the movement's ability to produce theories and policies that transcended local traditions, needs, and expectations.

This reader looks forward to the forthcoming two volumes of *L'Italia dei lumi*. Venturi's publisher, Einaudi, should share the credit for this ambitious and meticulously edited work.

CLARA M. LOVETT

George Mason University

JOHN MARTIN ROBINSON. *Cardinal Consalvi, 1757–1824*. New York: St. Martin's. 1987. Pp. xii, 212. \$24.95.

Although historians from Leopold von Ranke on have recognized his importance, no scholarly biography of Ercole Cardinal Consalvi (1757–1824), secretary of state to Pope Pius VII (1800–23), has yet appeared. As some compensation for scholarly neglect, the drama of Consalvi's life has inspired, every thirty years or so, biographies in a more popular mode, narrative rather than analytical, based on limited sources, and stressing the dra-

matic and picturesque aspects of his career rather than its deeper significance. John Martin Robinson's is the latest of these.

Consalvi took office in 1800, at one of the darkest moments in recent papal history: revolutionary France had occupied Rome, proclaimed the end of the temporal power, carried Pius VI off to die in captivity, and dealt deadly blows to a spiritual authority already undermined by the enlightened despots. Consalvi deserves much of the credit for the revival that now began. Not the liberal depicted by later admirers but an intelligent conservative, he was a pragmatic realist who saw that only if the papacy adapted as far as its essential nature allowed to the new world emerging from the revolution could it hope to survive. The centerpiece in that adaption was his concordat with Napoleon, which not only allowed the revival of Catholicism in France but gave Rome an unprecedented authority over the Gallican church. After 1801 he skillfully resisted Napoleon's efforts to dominate and manipulate the papacy. At the Congress of Vienna he secured the return of the Papal State—the achievement that, though ultimately futile, convinced contemporaries that he was a great statesman. During the Restoration he successfully guided Rome through the problems created by the revolutions of 1820 and negotiated a series of concordats that improved church-state relations and paved the way for the establishment of centralized papal authority over the church. He was less successful in his farsighted effort to place the Papal State on a solid basis of popular support by a program of moderate reform. His reforms were considered too radical by the Roman ruling class, which revoked them and drove him from office after the death of Pius VII in 1823.

Robinson's biography is written in a lively and anecdotal style and provides a generally accurate, if not very deep, account of Consalvi's career. He deserves credit for using the key archival source, the Fondo Consalvi in the Archives of the Propaganda Fide, previously consulted only by Lajos Pasztor and me. Robinson seems unaware, however, of most recent work on this period of papal history, by such historians as Pasztor, Raffaele Colapietra, and Anton van de Sande, who might have given him a deeper insight into Consalvi and his age. In sum this is a good popular biography that will please the general reader, but historians seeking an analytical scholarly biography that uses all the necessary sources—which is what Consalvi really deserves—will have to wait a while longer.

ALAN J. REINERMAN

Boston College

ANGELO ARA. *Fra Austria e Italia: Dalle Cinque Giornate alla questione alto-atesina*. (Civiltà del Risorgimento, number 23.) Udine: Del Bianco. 1987. Pp. 345. L. 25,000.

This volume brings together eight essays published between 1973 and 1981 in journals and elsewhere. They range from a detailed study of Count Karl Ficquelmont's reports to Klemens von Metternich from August 1847 to March 1848 on conditions in Lombardy-Venetia to an analysis, based on Austrian consular dispatches to Vienna, of the situation in Trieste during the 1920s and 1930s. Each offers a discrete, self-contained examination of an aspect of Austro-Italian relations during a crucial hundred years, in the course of which the relationship between the two countries was altered. Austria went from a powerful multinational empire, dominating the Italian peninsula, to a small republic. Italy achieved national independence and statehood, and, under Benito Mussolini, attempted to oversee Austrian affairs.

Angelo Ara, author of studies on Trieste, Italian irredentism, and Austria, demonstrates a mastery of Austrian sources. Not only has he scoured the archives but he also reveals familiarity with the press. The essay on Austrian reactions in the 1870s to Italy's internal affairs uses both diplomatic correspondence and leading Austrian newspapers to show how Austria interpreted them. Italy's policy toward the church, the problems of southern Italy, and, above all, the coming to power of the Left in 1876 received careful comment and analysis by Austrian diplomats and journalists.

Similarly, a survey of Italian attitudes toward Austria from 1848 to 1918 rests on solid research. Ara brings out clearly the ambivalence among Italians regarding Austria. Conservatives accepted Cesare Balbo's assumption that the basic conflict between Austria and Italy derived from Italian national aspirations. Once these had been satisfied with the formation of the Italian state, the two countries had no further quarrel. Moreover, conservatives viewed the Habsburg empire as a stabilizing force in European affairs and a bulwark first against Russian penetration from the east and later against an aggressive Germany. Democrats and radicals believed, with Giuseppe Mazzini, that the Austrian empire was a political anachronism that violated the rights of its subject peoples and should be destroyed. Oscillation between these two positions dominated Italy's relations with Austria, with the conservatives in control until the turn of the century when Italy began to build bridges to France and its allies.

Not relevant to the book's main theme—Austro-Italian relations—is the essay on Austrian anti-

Semitism, which, moreover, offers little new information. Also of limited interest is the discussion of the activity and speeches in the Austrian Reichsrat of the Italian patriot and socialist Cesare Battisti just before the outbreak of World War I.

These essays represent learned, detailed footnotes to the history of Austro-Italian relations since 1848. Historians of either Austria or Italy, or of their shared experiences, should find something of interest in Ara's thoroughly researched and well-documented studies but little to change accepted interpretations.

EMILIANA P. NOETHER
University of Connecticut

RICHARD BELLAMY. *Modern Italian Social Theory: Ideology and Politics from Pareto to the Present Day*. Stanford: Stanford University Press. 1987. Pp. 215. \$35.00.

Richard Bellamy's range is impressive, his ambition laudable, his subject important, and his approach promising. Yet his book proves a major disappointment. It is surely an achievement to tackle—in one slim volume—Vilfredo Pareto, Gaetano Mosca, Antonio Labriola, Benedetto Croce, Giovanni Gentile, and Antonio Gramsci, the most significant Italian intellectuals between 1890 and 1950, as well as Norberto Bobbio and Galvano Della Volpe, leaders in subsequent Italian political discussion. And these often elusive figures invite precisely the approach the author promises, following Quentin Skinner, whose contextualist treatment of past political thought, avoiding reductionism and respecting difference, has enabled distant ideas to challenge and deepen our own understanding. In Bellamy's hands, however, this approach engenders a constricting thesis that often obscures or trivializes the ideas at issue.

Seeking contextual illumination, the author fastens on the well-known imperative to "make the Italians" after 1860, in light of the limits of political unification. The desire for social cohesion and a common identity, he contends, led social thinkers to concentrate on education, consciousness, and theory, at the expense of economic processes, political organization, and institutions. The result, Bellamy feels, has been an abiding inadequacy in the Italian tradition.

There is something to this argument, but it cannot bear all the weight the author places on it. The examples of Emile Durkheim and Friedrich Tönnies remind us that a concern for social cohesion and an interest in consciousness were hardly confined to Italy during this period. So it is not clear how much the Italian contextual theme explains. Conversely, to grasp the apparently distinct-

tive element in the Italian tradition, it is necessary to question these thinkers more deeply, in terms of wider problems in Western political culture.

Although his reconstructions of particular ideas are sometimes superb, Bellamy's contextualist theme makes him quick to criticize, and to delimit, each of his figures. Thus, he does not bring out the qualities that make them significant—and still challenging—within the larger Western context. Moreover, his criticisms are at best conventional and sometimes misguided. He starts nicely with the troublesome idealists Croce and Gentile, for example, but then echoes the standard charges that Croce limits us to passive acceptance or irrational whim and that Gentile subordinates free individuals to the state. He has not grasped how historical understanding, moral judgment, and individual agency intersect in Croce's historicism or how freedom, participation, responsibility, and power converge in Gentile's fascism. Because criticism promptly preempts the promised dialogue, Bellamy's encounter with the post-Hegelian thinking of Croce and Gentile does not lead him to question his own foundationalist assumptions (for example, pp. 105–06, 166–67); thus, most of his criticisms simply miss the point.

The alleged inadequacy of the Italian tradition leads Bellamy to recommend a political approach that abandons the quest for privileged theoretical principles and relies simply on the common experience of democracy. His position merits development, but his broad use of "theory"—conflating disparate uses of theorizing about disparate mental objects—unduly homogenizes the Italian tradition. Moreover, he simply confuses levels in charging that success for any of these theoretical projects would have rendered politics redundant. Croce sought to show that politics is endless precisely because it cannot be theory-bound, and even Gentile and Gramsci can be understood as seeking a new politics, fully congruent with human freedom. These thinkers hardly merit the last word, but the author's dismissive use of "theory" makes it difficult for us to hear them at all. Indeed, this whole account, despite moments of considerable promise, too often seems a mere parody of Italian social thought.

DAVID D. ROBERTS
University of Georgia

JAN DZIĘGIELEWSKI. *O tolerancję dla zdominowanych: Polityka wyznaniowa Rzeczypospolitej w latach panowania Władysława IV* [On Tolerance for Minorities: The Confessional Policy of the Commonwealth during the Reign of Władysław IV]. Warsaw: Państwowe Wydawnictwo Naukowe. 1986. Pp. 227. 280 Zł.

Although the popular view usually equates Poland with Catholicism, scholars have long recognized the complex and diverse religious history of this country. Particularly in the second half of the sixteenth and the early decades of the seventeenth centuries was there a degree of religious toleration in Poland not found elsewhere in Europe. Jan Dzięgielewski's carefully researched and argued book—a revision of his doctoral dissertation—analyzes the religious policy of the Polish-Lithuanian commonwealth during the crucial decade and one-half long reign of King Władysław IV Vasa. This was the period when the tradition of toleration began to give way to a policy of hostility toward, especially, the several Protestant groups.

This book (which is limited to the Christian churches and excludes discussion of Islam and Judaism) is organized into two parts. In the first two chapters the author treats the period from 1632 until the meeting of the Polish Sejm in 1635, by which time the religious dissidents in Poland found themselves in a significantly worsened position in comparison to their fortunes during the reign of the previous Vasa king, Sigismund III. The second part of the book deals in three chapters with Catholic-Protestant relations as a factor in state policy and with the commonwealth's policy toward the Orthodox church. One of the most useful contributions the author makes is to distinguish between a period of "intolerant" state policy toward dissidents from 1635 to 1643 (during which time the Raków Academy of the Polish Brethren and the Calvinist church in Wilno were closed) and a more irenic approach in the next five years. He attributes this change to the influence of the king and, by implication, shows thereby the effectiveness of royal authority in the face of the growing influence of the nobility (*szlachta*). In addition to a thorough control of the printed sources and of the relatively limited secondary literature, this book is characterized by effective use of archival materials. The *acta* of such Protestant groups as the Czech Brethren were particularly valuable to the author.

The importance of this book lies not in its evaluation of the religious issues debated and discussed in this period; this has been better accomplished by, for example, Lech Szczucki and others in Poland, A. Jobert in Western Europe, and G. H. Williams in this country. Rather, its significance rests in the careful analysis of the politics of religion in the commonwealth and in the balanced and insightful evaluation of Władysław IV. Dzięgielewski shows in detail how the king's opportunism and dynastic interests influenced religious policy. In addition the author is effective in showing how conflicts between the various Protestant and dissident groups contrib-

uted to their worsening condition in this period. Although this is not a very exciting book to read, it is a solid and useful scholarly contribution. The book is printed in a relatively large edition of 3,500 copies, but the absence of a summary in one of the Western languages will effectively limit its impact.

PAUL W. KNOLL
University of Southern California

DANIEL BEAUVOIS. *Le noble, le serf et le révizor: La noblesse polonaise entre le tsarisme et les masses ukrainiennes (1831–1863)*. Paris: Archives Contemporaines. 1985. Pp. 365.

When Russia acquired the provinces of Kiev, Podolia, and Volhynia during the second and third partitions of Poland, it came into possession of an area where the Polish nobility (*szlachta*), although a numerical minority, was socially and economically dominant. According to the empire's eighth census in 1834, 4,282,439 Ukrainian peasants, 457,212 Jews, and 410,547 *szlachta* resided there. However, by 1863 only 70,000 *szlachta* remained on the right bank Ukraine. The remainder, most of whom were poor, landless, and without acceptable papers legitimizing their gentry status, were forcibly reduced to serfdom, the justification being their purported participation in the November 1830 Insurrection. As Daniel Beauvois demonstrates in this masterly study, this was only a rationalization for the russification of the area and the denationalization of the Poles. Russian efforts to "normalize" social relations in this former part of the Polish-Lithuanian commonwealth meant reducing them to Russian norms, that is, depriving the *szlachta* of their traditional rights and privileges, reducing the landless nobility (a social phenomenon hitherto unknown in Russia) to serfs, and transforming those who retained their noble rank into servants of the state.

The architect of depolonization was Dimitri Gavrilovich Bibikov, governor general of Kiev (1838–52), who created the Commission Centrale de Revision, which, between 1840 and 1846, declassified 160,000 of the 340,283 *szlachta* stricken from the rolls of the nobility and, thereby, deprived them of their legal rights between 1831 and 1856. In Beauvois's words, the object was "the destruction of an entire social group, of a type of civilization that, while certainly belonging to the past, was crushed only because it was Polish [and free]." History offers few examples of the destruction of an entire social group by bureaucrats and police, and this case bears witness to "the eternal power of Russia: the ability to eviscerate people without physical extermination" (pp. 152–53).

After discussing the liquidation of the most numerous group of *szlachta*, Beauvois candidly examines the social conditions on the estates of Polish landowners. Although not writing a history of Polish-Ukrainian relations, Beauvois demonstrates the trilateral (Ukrainian-Russian-Polish) nature of these relations, with the Russians inculcating into the landowners' thinking the idea that the Ukrainian peasants might be their economic property but not their cultural or national possessions. Analyzing the situation of the 70,000 "legal" *szlachta* in the final two chapters, Beauvois concludes that they functioned under a dual paralysis: the Sarmatian mentality of the wealthiest (who constituted only 10 percent of the "legal") and a threatening multifaceted terror pressed by tsarist authorities. In the first case the tsarist authorities, while exploiting the *szlachta* passion for titles and offices, spayed the *szlachta* dietines, transforming them into police tools and compromising the *szlachta* (for example, by having the dietines judge participants in the November Insurrection, pass on the declassification of fellow *szlachta*, support Russian schools, or select peasants for military service). Terror was applied through a variety of military, police, cultural, administrative, and religious measures, the most devastating of which were the confiscation of Catholic church lands and the liquidation in 1839 of the Uniate church, the symbol of Latin expansion in the East.

This study of the most extensive, and largely forgotten, effort at social manipulation prior to the twentieth century is based on the unpublished papers of the *szlachta* dietines, the correspondence of the marshals of the *szlachta* with landowners and the tsarist administration, and the chancelleries of the provincial governors general. It challenges the traditional bucolic and romantic Polish image of the Ukraine, as well as ideological studies that reduce Polish-Ukrainian relations to the progressive brotherhood of a handful of Polish-Ukrainian revolutionaries. Additionally, Beauvois corrects popular assumptions. It was accepted that the depolonization of the right bank Ukraine was largely accomplished by massive deportations after 1831. As Beauvois shows, massive deportations did not occur.

Some may conclude that Beauvois is emotionally and excessively sympathetic to the Poles. When one recalls Russian actions in eastern Poland between 1939 and 1941, however, it is hard to disagree with Beauvois's conclusion: "the Ukraine was a laboratory, and the Poles the guinea pigs in the first Russian attempt at digesting an enormous social group" (p. 348).

STANISLAUS A. BLEJWAS
Central Connecticut State University

BARBARA JELAVICH. *Modern Austria: Empire and Republic, 1815–1986*. New York: Cambridge University Press. 1987. Pp. xvii, 346. Cloth \$39.50, paper \$12.95.

This study of modern Austrian history covers primarily the nineteenth and twentieth centuries. The first half deals with the Habsburg empire with special focus on its German-speaking population, and the second half treats the problems and policies of the First Republic, the Anschluss and World War II, the four-power occupation and the State Treaty, and the Second Republic. Barbara Jelavich, a distinguished historian with a wide range of expertise extending from the Balkans and Austria to Russia and diplomatic history, has written previously on Austria in *The Habsburg Empire in European Affairs, 1814–1918* (1969) and again about it in parts of her two-volume *History of the Balkans* (1983). She treats Austria as an independent state, though a part of the wider German cultural nation.

Starting with enlightened absolutism, the author focuses on Klemens von Metternich, the revolutions of 1848, constitutional experiments culminating in the Ausgleich, the Dual Monarchy, and World War I. Thereafter, the treaty of St. Germain and the problems of the 1920s figure prominently in the narrative. The author is aware of the difficulties of treating a most significant period of Austrian history in a relatively condensed fashion, taking account also of cultural history and other aspects of its development. Still, there is a long chapter on Vienna as a cultural center before 1914. In 1932 Engelbert Dollfuss and thereafter Austrofascism came to power. Jelavich traces the turbulent history of Austria leading to Anschluss and war. Excepting the Carinthian and Styrian mountain areas, where Austrian Communists during the war established close relations with Yugoslav partisans, the author finds the extent of resistance in Austria difficult to determine. In any case the number of Austrians either executed or jailed for opposition to the Nazi regime “pales beside the figures on the fate of Jews, in whose persecution Austrians obviously cooperated” (p. 241).

Five years after the Anschluss, with the war approaching its last phase, the fate of Austria became once again a matter of international diplomacy. But Western policy toward Austria seemed one of “confusion and contradiction.” The Allied Moscow Declaration of November 1943 contained the “contradiction” of seeing Austria both as a “victim” of Germany and a German ally (p. 238)—a contradiction reflecting only reality.

Largely because of the cold war, the four-power occupation of Austria lasted a full decade. Aus-

tria's self-rule was finally restored, as was the political framework of 1920. The State Treaty of May 1955 forbade once again the Anschluss as well as the Habsburg restoration and also contained safeguards for the small Croatian and Slovenian minorities in Austria. The Second Republic's political development, problems, and accomplishments are treated in the last two chapters. Despite Austria's neutralization, welcomed by its citizens, the country has in fact become “an integral part of Western Europe,” sharing its political and economic philosophy. Neutrality has freed Austria's government from too deep involvement in the major East-West disputes. Political and economic changes have also moved both major parties toward the political center. Despite some very positive judgments about the Second Republic, the author ends on a cautious note: Austria's favorable situation can be maintained only if major political or economic crises are avoided, agreement on basic goals continues, and general tranquillity prevails in the world at large.

In a wide-ranging and broad study such as this, it is virtually impossible to find consensus by historians on all major problems. But the author has judiciously weighed pros and cons on all significant issues. The book is provided with a bibliography, deliberately limited to English publications, an index, numerous attractive illustrations, and a large number of maps.

ALFRED D. LOW

Bellevue, Washington

WALTER WILTSCHIEG. *Die Heimwehr: Eine unwiderstehliche Volksbewegung?* (Studien und Quellen zur österreichischen Zeitgeschichte, number 7.) Munich: R. Oldenbourg. 1985. Pp. 400.

Many of the different groups that made up the Heimwehr sprang up in the immediate postwar period when armed bands were formed to protect the fatherland from foreign foes, to ensure that the armed forces were demobilized in an orderly fashion, and to protect private property. Although they were avowedly apolitical, they were violently nationalistic and antisocialist and soon came into conflict with the republican army, which the later chancellor, Julius Raab, denounced as “Dr. Deutsch's hook-nosed guards.” Before long this pretense of standing above politics was dropped, and the Heimwehr devoted most of its energies to combating social democracy and then democracy itself, which the Heimwehr leader Richard Steidle insisted was “the screen behind which bolshevism hides.”

For all its cliché-ridden appeals for a “march on Vienna,” for the “authoritarian state,” for God,

freedom, fatherland, "universal fascism," and the spirit of the good old Ostmark, the Heimwehr had little idea what it was for, and its ideology was hardly more than a series of knee-jerk reactions to the political parties, "asphalt democracy," the "indolence and cowardice of the bourgeois," the "red peril," and "Asiatic influences." Politicians of the Right, such as Ignaz Seipel and Johann Schober, were tempted to use the Heimwehr, but it proved something of a sorcerer's apprentice. By 1930 the Heimwehr regarded the Christian Socials as just another contemptible bourgeois political party, and "one does not go to bed with someone who has the plague." The Heimwehr was greatly attracted to the National Socialist party, and there was much ideological common ground between them, but attitudes on both sides were ambiguous. The Nazis alternated between denouncing the Heimwehr as "cock's feather sadists" and treating them with contempt. By 1932 they were in direct competition with one another, much to the regret of the Heimwehr.

For a movement that placed such emphasis on soldierly virtues, parades, and uniforms, it was surprisingly lacking in military value. Training was inadequate, even in the handling of basic weapons, discipline was lax, and the command structure hopelessly inadequate. The leadership was second-rate. Ernst von Starhemberg, the "Austrian Hitler," was a profoundly silly political playboy. Steidle was a thoroughly unpleasant vulgarian with a certain talent for mob oratory. Dr. Walter Pfrimer was an effective organizer but lacked any moral fiber. Emil Fey was a brute whose boorishness was sometimes excused because he was of Saxon origin and none of the much vaunted Viennese charm had rubbed off on him. The Heimwehr was a disaster for the Austrian republic and had nothing to offer but the destruction of democracy. By 1934 that was effectively completed, and it had no further role to play. In October 1936 Kurt von Schuschnigg banned the Heimwehr. Its members offered no resistance, and many joined the Nazis, in whose ranks some were to make modest careers.

Walter Wiltschegg has written a most useful guide to this sorry tale. He emphasizes the regional differences, the ideological confusion, the organizational weaknesses, and the lack of effective leadership. The mass of detail, the long lists of names and offices, and the thematic rather than chronological organization do not make for easy reading, but the book does provide an invaluable fund of information on this important but curiously neglected subject. The author rightly bemoans the lack of idealism and the selfishness of our own day, but cautiously to praise the Heimwehr for possessing such qualities seems out of

place. Idealism that does not serve a noble end is at best worthless, at worst dangerous. The idealism of the Heimwehr falls into the latter category.

MARTIN KITCHEN

Simon Fraser University

DAVID ENGEL. *In the Shadow of Auschwitz: The Polish Government in Exile and the Jews, 1939–1942*. Chapel Hill: University of North Carolina Press. 1987. Pp. xii, 338. \$29.95.

David Engel has produced a detailed study of the formulation of policy toward the Jewish Question by the Polish government-in-exile in the period September 1939 to December 1942. He has based this study on painstaking research in the voluminous papers of the Polish government (archives of the Hoover Institution for War, Revolution, and Peace; the Sikorski Institute and Polish Museum; and the archives of the Polish Underground Movement; British Foreign Office papers, Public Record Office; and the Jewish archives in Jerusalem and Tel Aviv). Finally, he has studied selected Polish and Jewish newspapers and conducted interviews with the former Polish ambassador in London, Edward Raczynski, and with Jan Karski, who journeyed twice as a courier from occupied Poland to England and on the second occasion brought detailed information of the German genocide of the Jews in Poland (November 1942). The present volume is the first of what is to be a two-volume study of the subject.

Engel tries to present a balanced picture of the evolution of Polish governmental policy toward the Jewish question but tends, nevertheless, toward a negative evaluation. The documentation he adduces does not bear out his contention that the government still followed the prewar tradition of excluding the Polish Jews from the body of people it undertook to serve. Like any other government, the Polish one was concerned primarily with the majority of its people and not with the Jewish minority, a minority, moreover, that was both much larger than in any other country (10 percent of the population) and radically different from the assimilated Jews of Central and Western Europe and the United States in having a distinct culture and way of life. However, when the government received the first report of genocide—in a letter from Bund leaders in Poland—it took vigorous action to publicize it and did so in June–July 1942, that is, before Karski arrived in mid-November with additional and personal evidence, after which the government sent its first official note on the subject to the Allied governments.

It is surprising that Engel accuses the Polish

government of "deliberate prevarication" in this latter instance (p. 197). It is not clear whether he means the lapse of one month between Karski's arrival and the Polish note or the lack of significant action in the period July–December 1942. In any case, he admits that a review of the correspondence directed by Jewish leaders and organizations to the Polish government in the period from June to mid-December 1942 (that is, between the publicity given the Bund letters and the Polish note to Allied governments) shows their major concern remained the status of Jews in postwar Poland, discrimination against Jewish soldiers in the Polish army, and Jewish refugees (pp. 303–04, n. 185). He admits the possibility that the Polish government simply could not believe what was happening but does not see that this might also apply to Jewish leaders and organizations, thus explaining their lack of pressure on the Polish government to take further action and, by the same token, the lack of significant Polish official action at this time.

Another complex and controversial question concerns Polish resentment against Jewish cooperation with Soviet authorities in eastern Poland, which led to increased anti-Semitic feelings both in German-occupied Poland and later in the Polish army in the Soviet Union. In citing Karski's 1940 report on this matter, Engel calls the Polish charges of Jewish-Soviet cooperation "an unwarranted extrapolation from a fundamental kernel of truth," since the Jews in the east welcomed the Soviets as protectors from German subjugation (p. 61). Although this question requires a study on its own, the reports from both eastern Poland and Lithuania speak not merely of the Jews "welcoming" the Soviets, which is not surprising in itself, but of many young Jews assuming police and administrative positions; worse still, there are reports of their cooperation with the NKVD in tracking down and arresting Poles wanted by the Soviets. It is difficult to say just how much this news influenced Polish attitudes toward Jews in German-occupied Poland in 1939–41—that is, before the Germans began to implement their genocide program—but it certainly had a great impact on the soldiers of the Polish army in the Soviet Union in 1941–42 and also in the Polish army in the west. Combining this issue with the facts that the leadership of the prewar Polish Communist party was largely Jewish and that once the Soviet Union became an ally of Great Britain and the United States most Jewish organizations in the west refused to support the Polish government in its stance on the Polish-Soviet frontier and, in fact, gave financial and press support to Moscow, it is not difficult to see why there were instances of discrimination against Jewish soldiers. Even so,

Engel admits that the Polish government took measures against such discrimination.

It is impossible to list all the controversial aspects of this study, but it is necessary to take issue with one theme that pervades the book. The author seems convinced that what he calls the old Polish belief in the ineluctable hostility or threat of "world Jewry" toward Polish independence continued to exist in the Polish government-in-exile. Here he confuses a belief common to the majority of National Democrats—but not held by Polish Socialists or Piłsudskiites—and the belief of General Władysław Sikorski, head of the government-in-exile, and his closest advisers that "world Jewry" and, especially, Jewish organizations in the United States were a power to be reckoned with and that their support was therefore desirable. As it turned out, Sikorski and his advisers vastly exaggerated the power of U.S. Jews, who, in any case, decided not to exert pressure on President Franklin D. Roosevelt to rescue the Jews of Europe for fear that an influx of European Jews might provoke an increase of anti-Semitism in the United States.

Among several errors to be found in the book, it should be noted that the concept of a government-in-exile was not a "Polish invention" (p. 230, n. 27). In fact the Polish government was based on the precedent set by the Belgian government that transferred to France in 1914. Also, after the fall of Norway, Holland, Belgium, Greece, and Yugoslavia, they were also represented by governments-in-exile in London, while the Free French had their representation in the committee headed by General Charles de Gaulle. Sikorski's government cannot be accused of following *Sanacja* traditions in being only "quasi-parliamentary" in nature because the National Council was only an advisory body (pp. 51–2). Granted that Sikorski sometimes acted on his own, we should note there could be no full parliament-in-exile, and no other exile government had one either. But we should also note that Poland had an underground national council as well—something that no other exile government possessed—and paid close attention to its views. Foreign Minister Józef Beck's repudiation of the Minority Treaty in 1934 had nothing to do with the Polish-German Declaration of Non-Aggression signed earlier that year, which did not replace the Franco-Polish alliance whose existence was recognized in the declaration (p. 36). Raczyński was the "architect" in this move in the sense of preparing it, but it was decided by Beck and Piłsudski in late 1932 and carried out in 1934 with the goal of preventing Soviet-inspired action in Poland once the Soviet Union entered the League of Nations, which it did in September of that year, after giving the required pledge to Poland. Roman Knoll was not the Polish ambassa-

dor in Berlin (p. 65) but the Polish envoy there in 1928–29, because the Polish legation in Berlin and the German legation in Warsaw were not raised to embassy rank until 1933. It is not true that the British had “little esteem” for General Sikorski in July 1940 (p. 69). Frank Savery’s remarks were made at a time of government crisis, and the British government, unlike Savery, who disliked him, threw its support to Sikorski, who retained his position. The “bands of toughs” who harassed Jews in German-occupied Poland (p. 161) were selected and paid by the Germans, and these actions were condemned by Polish opinion. Engel does not mention the fact that on October 15, 1941, the head of the General Gouvernement of occupied Poland, Hans Frank, proclaimed the death penalty for Poles who hid Jews, which was quite unnecessary if Poles were not helping Jews. Finally, the author does not mention that a special organization to help Jews, called “Żegota,” was set up by the underground in the fall of 1942.

ANNA M. CIENCIALA
University of Kansas

PEDRO RAMET. *Cross and Commissar: The Politics of Religion in Eastern Europe and the USSR*. Bloomington: Indiana University Press. 1987. Pp. x, 244. \$22.50.

There is a specter haunting the Communist regimes of Eastern Europe. It is the specter of religious revival. As Pedro Ramet’s excellent study notes, a wave of religious ferment has swept the region since the 1970s that “presents the communist authorities with a challenge as trying and as complex as the multifaceted economic woes currently afflicting the region” (p. 130). Anyone trying to understand either this unusual development or the recent history of Eastern Europe (including the Soviet Union) will find this book’s effort to systematically analyze the contemporary politics of religion of the region and to place its church-state relations into a theoretical framework very useful.

Ramet’s basic premise is that a variety of social science approaches can be used to elucidate church-state interaction in this area in a manner that goes beyond the purely factual or apologetic level now common. His study is organized into four sections. Part 1 begins with an excellent summary of the problems, complexities, and nuances of church-state coexistence under Communism. One major theme is the importance of the unique “operational ambivalence” of Communist regime policy that rejects religion outright and at the same time uses religious organizations as policy tools. From this ambivalence result gyrations of

regime policies vis-à-vis religion and apparently perplexing variations across time and locality and toward particular groups. The remainder of part 1 deals with the effects of modernization on church-state relations and with the interplay of religious and nationalities policy.

Part 2 moves on to applying a number of theoretical strategies (functional analysis, historical analysis, political culture analysis, factional analysis, and organizational theory analysis) to different church organizations and situations in Eastern Europe. The countries analyzed are the Soviet Union, Poland, Czechoslovakia, Hungary, the German Democratic Republic, Yugoslavia, and Bulgaria. The purpose of this section is to use what the author calls middle-range theories to provide for a variety of complementary perspectives on church-state interaction. The cases are well made; the results are illuminating.

Part 3 is even more specific, using simple, lower-range hypotheses to investigate, first, the effects of the election of Pope John Paul II on the religious renaissance in Eastern Europe, and, second, the interplay of accommodation, coexistence, and confrontation between church and state in the region. This effort to seek out patterns in current developments successfully describes the broad influence of the Polish pope on the east as well as the challenges facing both church and state in their evolving relations.

Part 4 returns to high generalization. A salutary discussion is provided concerning “pitfalls in the study of church-state relations,” which should be required reading for specialists on the topic. The section concludes with the presentation of a matrix or theory of church-state relations under Communism that summarizes the study, including the problems caused by modernization for both churches and regimes, the interplay between nationalism and religion, the role of historical experiences in understanding church-state relations, the relationship of religious culture to political culture, the importance of factionalism in the politics of religion, and the uses of organizational theory in showing the origins of policy outcomes in church-state interactions.

Ramet is fair in his analysis while avoiding the error of letting explanation emerge as justification. He certainly does not mince words in evaluating church-regime interactions in Eastern Europe and the Soviet Union, but he refrains from polemic. He is obviously familiar with the theoretical tools he applies in his analysis. At the same time he has previously done extensive scholarly work on and in most of the countries under consideration. (Romania appears to be a significant exception and is perhaps the weakest part of the book.) Ramet also takes what one might call a

common sense view of theory and historical study: "it is interesting when the questions it raises are useful" (p. 177). Thus, the purpose of theory "is not to constrain the subject matter into a conceptual straitjacket, but rather to open new avenues for understanding" (p. 195). This becoming modesty and sensitivity, coupled with the thoroughness of his explorations, contribute to a successful product that both enlightens and informs.

PAUL E. MICHELSON
Huntington College

MARGARETA MOMMSEN. *Hilf mir, mein recht zu finden: Russische Bittschriften von Iwan dem Schrecklichen bis Gorbatschow*. Berlin, F.R.G.: Propyläen. 1987. Pp. 343. DM 68.

Firmly embedded in Russian tradition, the unwritten right to petition the crown perished in January 1905, victim of the crown's right to punish the petitioner. One way to avert a repeat of Bloody Sunday, Nicholas and his advisers reasoned, lay in not enshrining the ancient privilege in the new state order. The deliberate omission, as Margareta Mommsen points out, figured in Max Weber's dismissal of the post-1905 system as "pseudoconstitutionalism."

Mommsen begins at the beginning, with the establishment of the *chelobitnyi* (the etymology follows that of "kowtow") *prikaz* early in the sixteenth century but devotes relatively little attention to the period down to 1864. Relying chiefly on the works of a handful of tsarist and Soviet historians, she does, however, succeed in setting the scene and providing continuity for an analysis of the Soviet period, to which she devotes more than half her book. Given the promise of the title, some readers will regret the sparse discussion of the fate of petitions and petitioners in the reigns of powerful rulers such as Ivan IV and, especially, Catherine II, whose disposition of the problem some Soviet rulers have endorsed by imitation. Mommsen does pay close attention to Aleksei Plavilshchikov's pioneering work (1811) on petitions in the period following the *Sobornoe Ulozhenie* of 1649, and she relates the history of institutional developments in the nineteenth century in revealing detail.

Although a number of institutions dealt with petitions and anonymous letters, in the period 1826–1880 the Third Section, empowered to investigate "all occurrences without exception," carefully examined most unsolicited communications to state officials and agencies. The section's creator, Nicholas I, recognized petitions as a rough guide to public opinion and treated them accordingly. Peasant complaints about landlords indicated a laxity of discipline best corrected by

lashes some of the petitioners, dragooning others into lifetime military service, and, if need be, quartering troops in the affected area. Alexander II modified this policy substantially; the backward-looking last two tsars could not fully restore it.

The Soviet rulers of Russia, as Mommsen demonstrates, have pursued a mixed policy. Petitions have often taken the form of a letter to a newspaper, which Mikhail Kalinin pronounced "no longer a private letter [or] . . . an individual complaint, but a document!" The Worker-Peasant Inspectorate (Rabkrin) came into being in 1920 in part to deal with the flood of such documents and others sent directly to the authorities; Feliks Dzerzhinsky and the secret police could not then cope with the task unaided. Rabkrin's demise in 1934 indicated not only Stalin's dissatisfaction with its work but also the coming of age of Dzerzhinsky's organization, renamed NKVD in the same year.

Some petitions and appeals raised serious issues of more than local significance; many more dealt with trifles and often reflected personal quarrels. The evidence Mommsen has assembled indicates that, at least until the Great Terror, the authorities reacted with considerable sensitivity to these communications. Documenting the period 1936–39, when anonymous denunciations led to the incarceration and death of uncounted masses of innocent citizens, remains a virtually insurmountable task. Not even the Smolensk Archive, so effectively employed elsewhere in this book, is of much help. Most documents went to Moscow, and most have been destroyed by the KGB.

The citizenry's exercise of the right of petition played an important role in "de-Stalinization" but failed to halt the pervasive corruption of the Brezhnev regime, which contemptuously assigned the number fifty-eight to the article of the Constitution of 1977 guaranteeing the right to file suit against officials and agencies and to compensation for damages. Millions of Soviet people disappeared into the Gulag—often following an anonymous denunciation—under the infamous Article Fifty-eight of the nongeneral division of the Criminal Code of 1926.

The era of *glasnost* and *perestroika* has produced the astonishing phenomenon of the state literally pleading with its citizens to submit petitions with constructive criticism. The new leaders profess gratification at the response. Mommsen shows in this excellent study, however, that the crucial role of the right of petition in Russian history indicates nothing so much as the weakness of Russia's judicial institutions.

WOODFORD MCCLELLAN
University of Virginia

J. E. O. SCREEN. *The Helsinki Yunker School, 1846–1879: A Case Study of Officer Training in the Russian Army*. (Studia Historica, number 22.) Helsinki: Suomen Historiallinen Seura. 1986. Pp. 319.

J. E. O. Screen presents a meticulous study of an institution that in its brief existence graduated 940 men with the right of promotion to officer. The Helsinki Yunker School was never an important source of officers for the tsarist army. On the other hand, as Screen notes, the yunker schools as a group were for a time the largest single source of officers, and in the 1860s and 1870s the curriculum at Helsinki was much like that elsewhere. Screen details the teaching program, the examination schedule and average examination results, the rate of promotion from one class to the next, the graduation rates, and much else besides. In some periods a good deal more than half of those admitted did not stay the course, which the author attributes to the inadequate educational background of entrants. He shares the conclusion of the inspectors' reports that the educational program was good given the quality of the students. Unfortunately, Screen makes no consistent effort to gauge the adequacy of the educational program to the military requirements of the day or to the experience graduates would face in the regiments, nor does he have much to say about what the program might tell us about the expectations that military authorities had of Russian officers. He focuses steadily on the Helsinki school itself, so we learn a great deal about its finances, even about its bathhouse, stables, and armorer's shop.

The yunker schools became a crucial element in the system of officer education during the 1860s, when they were established to provide formal instruction to men who would otherwise become officers merely by combining the proper social background and service in the ranks with the passing of none-too-rigorous exams. The Helsinki school predated the reform period and was initially intended to provide schooling in Russian for Finns who wished to become officers. The school never served Finns exclusively, but it did throughout its existence make special provisions for Finns and therefore received a subvention from the Finnish Senate. Without the annual Finnish subsidy, the school would neither have been set up nor continued. When in the 1860s the curriculum was aligned with that of other yunker schools and especially when the school expanded and its students became overwhelmingly Russian in the 1870s, the Finnish Senate lost interest. A wrangle over paying for new premises led to its closing in 1879. The school thus occupies a modest place in the story of Finland's relations with Russia, and it

is this facet of the story that Screen best illuminates.

The study in some ways resembles memorial histories of military schools and regiments that present information for preservation's sake. The lists of teachers and the summaries of correspondence about small amounts of money make tedious reading (but they do illustrate just how tight-fisted the Russian military could be in small matters). Nevertheless, much diligent research—the primary documentation is in Russian, Swedish, and Finnish—has gone into this book, and it will be useful to anyone who wishes to understand Russian officer education.

JOHN BUSHNELL
Northwestern University

A. V. IGNAT'EV. *Vneshniaia politika Rossii v 1905–1907 gg.* [Russian Foreign Policy from 1905 to 1907]. Moscow: Nauka. 1986. Pp. 300. 2 r. 80 k.

The author, whose publications have already covered Russian foreign policy in the decade immediately following the years treated in the present work, has herewith contributed to a historiographical inventory that includes many Soviet writers. This comprises such well-known works as those of B. A. Romanov and the more recent study of the Anglo-Russian agreement by A. F. Ostal'tseva. In an introductory chapter A. V. Ignat'ev comments on many of these studies, mostly Russian but also a number of foreign works. His own study is chronologically more restricted than most of these but at the same time more inclusive than some in its collateral scope.

This monograph deals with the domestic and international redistribution of influence and power that followed the Russian defeat in the war with Japan and with the concomitant increase in the foreign threat to the Russian imperial regime, particularly that of Germany and Japan. The author identifies Germany as the greatest gainer from this shift in power and sees France and Britain as the nations on which Russia was forced reluctantly to rely in its endeavor to correct this unfavorable diplomatic balance. He expresses the view that Russia would have preferred to have remained without obligation to either side of the diplomatic dichotomy, thus going a step beyond Count Vladimir Lamsdorff's earlier dictum that Russia should remain equidistant from Britain and Germany. Either of these formulations had of course to be adjusted to accommodate the entry at that time of Japan into the ranks of the great powers.

The book is divided into two major sections, one chronological and the other topical. The chrono-

logical part begins with a chapter dealing with the international position of Russia after the conclusion of the Portsmouth treaty. The author then turns to an examination of the period from the Portsmouth treaty to the spring of 1906 when the Anglo-Russian negotiations were formally launched. The next period is seen by the author as encompassing the remainder of 1906. It is characterized as a time when there was a national effort to recoup the damaged international position of the country by internal reconstruction and by restoring the aspects of relations with Japan that had not been settled at the Portsmouth conference. The final months discussed in the study constitute most of 1907, during which actual settlements were made with Britain and Japan and attempts were made to restore the mutuality of the Franco-Russian alliance and to return to a working relationship with Germany. The second major section of the book is topical and deals with some regional aspects of the postwar situation such as those concerning the Balkans and the Middle and Far East.

The era about which the author writes was a time of domestic revolution and international diplomatic revolution. He sees the enhanced position of Germany as of the greatest significance for Russia, not only on the European continent but also in regions peripheral to both countries. One of these areas of rivalry was the Baltic Sea, where the usability of this outlet became complicated by the separation of Norway and Sweden into separate, rival nations, the revival of the Åland Islands issue, the emergence of German naval power, and the further evolution of Anglo-German naval rivalry. The issue had been dramatically raised in October 1904 by the passage through this region of the Russian Baltic fleet en route to the Far East.

The author has identified the significant issues of the era and, with the help of a considerable number of both primary and secondary materials, has marshaled a great deal of information about these issues. He has not, however, been uniformly successful in bringing the evidence into meaningful focus with the issues. This is true to some extent with respect to his characterization of the Austrian-sponsored endeavor to reconstruct the 'Three Emperors' League as a counterpoise to the attempt to fashion an Anglo-Franco-Russian combination. It is true to an even greater extent in his endeavor to deal with the bitter Russo-German antagonism over the Algeiras conference, a crisis that was potentially dangerous.

JOHN A. WHITE
University of Hawaii

I. N. IONOV. *Profsoiuzy rabochikh Moskvy v revoliutsii 1905–1907 gg* [Workers' Trade Unions in Moscow during the Revolution of 1905–07]. Moscow: Nauka. 1986. Pp. 166. 1 r. 80 k.

Perhaps it is a sign of *glasnost* that Soviet historians are at last attending to the history of prerevolutionary trade union organization. If this tendentious and uninspired monograph on the birth and early development of unions in Moscow during the upheavals of 1905–07 suggests the quality of their work, however, further *perestroika* is sorely needed. To be sure, the book has a few partially redeeming features. Not only do unions share the spotlight with the Bolshevik party but groups of workers infrequently studied in Soviet literature—printers, tailors, bakers, municipal employees—refreshingly overshadow the usually omnipresent representatives of the “cadre” industries, textiles and metalworking. I. N. Ionov even dares to tackle—briefly and inadequately—the delicate problem of *tsekhovshchina*, or craft (literally, shop) consciousness. Nevertheless, the book is so unimaginatively conceived and ineptly argued, even when compared solely to previous Soviet scholarship, that no new insights and little information may be gleaned from it.

Ionov contends that Moscow's unions emerged as by-products of revolution, not as institutions defending economic interests as in the West; they were “proletarian organizations of a new type,” hostile to “trade unionist” ideology and closely tied to the Bolshevik party. This is only partly true. That Russian unions did not develop according to the classic Anglo-American model is undeniable, as is the fact that union organization was closely intertwined with socialist political agitation. That Bolshevik Social Democrats were from the start the sole leading force in the union movement, however, is arguable only through the most selective presentation, if not willful distortion, of facts. Many working-class Social Democrats at this time were nonfactional, and those with factional affiliation frequently cooperated with each other as well as with independent unionists and Socialist Revolutionaries.

Development of the early unions is traced through four phases, but much of the account bogs down in often unsubstantiated generalizations about the revolution as a whole and tedious exegeses of Leninist policy. Aside from the continued assertion—with little evidence—of Bolshevik influence, no consistent argument ties together the story of the unions themselves. There is little discussion of the functioning of these infant institutions and only the most peripheral and trivial treatment of their leading activists. Although Menshevik influence is acknowledged among the

printers, their union and those of others are discussed mainly to shed light on Bolshevik successes. The national railroad and postal-telegraph unions, whose Moscow activities were pivotal in 1905, are given relatively short shrift, with crucial facts ignored or misrepresented.

Ionov criticizes the "falsifications" of "bourgeois" historians Victoria Bonnell and Laura Engelstein, but their fine books on worker organizations and on 1905 in Moscow remain far superior to this poor example of Soviet scholarship.

HENRY REICHMAN

Memphis State University

A. K. SOKOLOV. *Rabochii klass i revoliutsionnye izmeneniia v sotsial'noi strukture obshchestva* [The Working Class and Revolutionary Changes in the Structure of Society]. Moscow: Moskovskoe Universitetskoe Izdatel'stvo. 1987. Pp. 226. 2 r. 70 k.

Soviet historiography has shown in recent years a few, timid signs of the "openness" championed by the Gorbachev leadership. The book under review here is an indication of the effort by some Soviet historians to introduce new conceptual paradigms in place of the dogmatic canons that have governed their discipline in the past half-century.

In this case the cause defended by the author is the "systems analysis of the social structure" of the Russian working class in the Soviet period (p. 46). His methodology relies primarily on computer-assisted statistical analysis of quantitative data. His didactic tone and somewhat random selection of subjects for analysis are reminiscent of the Western studies of the 1960s promoting the new social history. The author is careful to distance his "Marxist" conceptualization from that of "structuralist-functionalist analysis," which fails in his opinion to emphasize the "historical context [*obuslovlennost'*]" of each type of social structure" (p. 57). This bow to historical materialism signifies little more than a statement of reputability. In research design and analysis of findings, this book is remarkably free of Marxist-Leninist bibliolatry and historicism.

Like the early Western quantitative studies of social history, A. K. Sokolov's volume constitutes as much a handbook to inspire and to guide further research as a study of the history of the Soviet working class. One-third of the book is devoted to theoretical and methodological issues arising from research into the social structure of the working class in the "transitional period from capitalism to socialism." The first two chapters include a thorough review of recent Western literature on Soviet social history and on modernization and explore the complexities of correlation

analysis and the construction of machine-readable files. These methods are then applied directly to aggregate data drawn from historical sources appropriate to quantitative analysis of the Russian working class in the early Soviet period.

The topics chosen for study are the structure of the working class in the early Civil War years, the social development of the working class from the revolution to the First Five-Year Plan period, and the social profile of the worker-activists participating in the first congresses of soviets under Communist rule. Using manuscripts from the 1918 occupational census, the author employs statistical tests to search for significant groupings of variables (extracted from correlation matrices) that identify particular segments of the worker population. Sokolov is inspired by his findings to construct a complex "schematic model of the formation of the social structure of the working class" (p. 116). Historians who enjoy sociological puzzles may find it challenging, but those skeptical of deterministic modeling in historical analysis are unlikely to be convinced that it is any improvement over more intuitive theorizing.

The study of worker-activists draws its findings from the detailed surveys prepared by all delegates to the All-Russian congresses of soviets until 1922. It seeks to define, in a summary form, the "life history" of these Communist supporters. Not surprisingly, it reveals that prerevolutionary activism constituted the single most important characteristic of the delegates. If Sokolov's exhaustive quantitative analyses lead to common-sense conclusions, his experience would seem to bear out observations made in the West regarding computers and historical research. Gaps in our knowledge of the history of the Soviet working class will disappear thanks to such Herculean labors, but our understanding of the role of the working class under the Communist regime will be determined largely by theoretical and conceptual debates that no statistical procedures can resolve.

DANIEL R. BROWER

*University of California,
Davis*

S. N. BAZANOV. *V bor'be za oktiabr' na severnom fronte: 5-ia armia* [In the Battle for October on the Northern Front: The Fifth Army]. Moscow: Nauka. 1985. Pp. 158. 1 r. 30 k.

L. M. GAVRILOV. *Soldatskie komitety v Oktiabr'skoi Revoliutsii (deistvuiushchaia armia)* [Soldiers' Committees in the October Revolution (the Frontline Army)]. (Istoriia Velikoi Oktiabr'skoi Sotsialisticheskoi Revoliutsii.) Moscow: Nauka. 1983. Pp. 220. 2 r. 60 k.

M. M. GITSIU. *Deiatel'nost' soldatskikh sovetov i komitetov na rumynskom fronte i v Moldavii v 1917 g.* [The Activity of Soldiers' Soviets and Committees on the Romanian Front and in Moldavia in 1917]. Kishinev: Shtiintsa. 1983. Pp. 158. 1 r. 90 k.

The above three works represent recent Soviet scholarship on the front army during the revolution of 1917. They follow the pattern of earlier publications by such authors as M. I. Kapustin, N. M. Iakupov, P. Golub, and V. I. Miller in treating particular front sectors, time periods, or themes (the party, soldiers' committees). In this case the works of S. N. Bazanov and M. M. Gitsiu cover the Fifth Army and the Romanian front, respectively, and L. M. Gavrilov continues the work of Miller on soldiers' committees. What promises to make these works more valuable than previous works is the much higher standard in the use of sources (the military archives and the rich legacy of the scholarship of the 1920s) already set by Miller in *Soldatskie komitety v russkoi armii v 1917 g.* (1974) and T. F. Kuz'mina in *Revoliutsionnoe dvizhenie soldatskikh mass tsentra Rossii nakanune Oktiabria* (1978); moreover, in the meantime three more substantial volumes of documents on the front have appeared covering the October revolution, military revolutionary committees, and soldiers' committees in the editing of which Gavrilov played a major part. The Fifth Army of the northern front is a particularly crucial selection in that it was one of the most turbulent in 1917, being most exposed to influences from the revolutionary capital, and was the only army in which the Bolsheviks acquired a majority at an army congress prior to the Second Congress of Soviets. The Romanian front is interesting for the opposite reason: it was remote from the urban centers, and it was the least bolshevized at the time of the October revolution. (Ukrainians greatly outvoted the Bolsheviks in the Constituent Assembly elections.) Gavrilov's work promised to supplement the important contributions of Miller, who carried his treatment only through the month of April.

Given these expectations, the present works historiographically represent a slip backward to the stereotyped treatments of former times. The greater use of archival and published sources only serves to pile on more examples of the standard liturgy, always prefaced by citations from Lenin and the pat formulas of the soldiers in every revolutionary action being guided by the conscious Bolshevik vanguard, regardless of whether it is indicated by the cited sources or not. The old error of selecting out examples that maximize the Bolshevik presence and influence mars each of these works. Gavrilov has the irritating habit of illustrating each point, fraternization for example,

by citing the instances of the well-known Bolshevik units first (438th Novoladozhskii Regiment of the Twelfth Army, 120th Division of the Fifth Army, the Transamur divisions in the Eighth Army) always beginning with the 42nd Corps, which was stationed in Finland and not on the front and had always been a part of the metropolitan nexus of Bolshevik influence. The result, of course, is greatly to magnify the impression of Bolshevik initiative and leadership even where not documented or probable. Perhaps the most striking instance of this kind of distortion in each of the works is the treatment of the massive front reaction to the Kornilov affair, which was indeed a major landmark in the resurgence of bolshevism. However, my researches indicate that this resurgence postdated the Kornilov affair, though greatly facilitated by it, but most of the revolutionary actions of taking over army and front headquarters, arresting officers, and forming of special controls over communications were carried out either spontaneously by soldiers or jointly with commissars and committees of Socialist Revolutionary and Menshevik composition. Nevertheless, each work follows the ritual formula of "the revolutionary soldiers, led by the Bolshevik party, thwarted the plans of the counterrevolutionaries," although it is exceedingly difficult to find even individual instances where there was an effective Bolshevik role.

It is interesting that the two case studies do bring out facts (almost unavoidable) that indicate that the Bolshevik political revival at the front was due far more to the perceived opportunities of the Constituent Assembly elections than to the Second Congress of Soviets, for most of the energies of organized Bolsheviks, including the "advanced" ones of the Twelfth and Fifth Armies, were devoted to drawing up lists of nominees for the elections. Only after the old Soviet Executive Committee announced the calling of the Second Congress around October 1, did the attention shift toward frenetic by-elections to soldiers' committees to secure Bolshevik representation at the congress. The fact that the southwestern and Romanian fronts were scarcely represented at the Second Congress because of the effective boycott of the old committees is passed over lightly in these accounts. One can establish this by careful attention to the facts cited by the two case studies, and that is to their credit, but in Gavrilov's work the political realities are obscured by general formulas.

All this is more the pity because Soviet scholarship has documented extensively what has been totally ignored in Western accounts, namely, that in late October and early November the Bolsheviks were scoring heavily in by-elections to sol-

diers' committees at the front, within two weeks of the October revolution were in complete control of the committee structure on the northern and western fronts, and had secured major victories in the Seventh and Eighth Armies on the southerly fronts, a cardinal development that made any reversal of the coup in Petrograd impossible. This development has been well covered in previous Soviet works (as part of "the triumphal march to socialism"), but the present works provide a good many more details.

In short, Western scholars still have a good deal to learn about the role of the front in the October revolution and should take Soviet scholarship seriously in reordering their own approaches, but the present volumes add only marginally to what has previously been achieved. Whether Soviet scholarship can be freed of some of its negative characteristics by the current trends toward liberalization remains to be seen, but thus far historical scholarship remains a bastion of conservatism and orthodoxy in contrast to the startling changes in the realm of the arts and culture.

ALLAN WILDMAN
Ohio State University

TIMOTHY EDWARD O'CONNOR. *Diplomacy and Revolution: G. V. Chicherin and Soviet Foreign Affairs, 1918-1930*. Ames: Iowa State University Press. 1987. Pp. xx, 250. \$24.95.

This monograph promises more than it delivers. Advertised as the "first western biography" of Soviet Russia's foreign minister in the 1920s, G. V. Chicherin, it is in fact a survey of early Soviet foreign policy with special attention to Chicherin's role as diplomat. Described as "based on research in Soviet archives and libraries," it actually depends mainly on recent Western studies and published Soviet documentary collections.

The 170-page essay deals with Chicherin's family heritage, his years as a revolutionary (Menshevik) émigré in Western Europe, and his role in establishing and running the Soviet Ministry of Foreign Affairs, Narkomindel, in the 1920s. Special attention is given to Soviet relations with Germany and Great Britain, as well as to Chicherin's relations with Stalin. Timothy Edward O'Connor concludes that Chicherin "had a significant impact on the conduct of foreign affairs" and made "an original and substantial contribution to the consolidation of the Soviet government in Russia" (p. 169). The first conclusion is more supportable than the second.

The Chicherin archival materials in Moscow and Leningrad yielded primarily information on the family and Chicherin's early years, virtually

nothing on early Soviet foreign policy. The value of these materials is often overestimated. For example nine successive footnotes cite the same ten pages of the State Archives collection in Moscow, repeating the entire archival fund citation in each footnote. Much of the rest of the book is heavily dependent on other Western monographs; the reader will often be better served by reading the works by Kurt Rosenbaum, Teddy Uldricks, and Richard Debo, for example, on Soviet-German relations, the beginnings of Narkomindel, and early Soviet foreign policy.

Despite passing disclaimers, Chicherin emerges as a diplomatic technician with very little, if any, influence on the deeper workings of the Politburo and policy making generally during the 1920s. He was overshadowed by Lenin and, after the latter's death, outmaneuvered by Maxim Litvinov. Separating biography from foreign policy remains an elusive task. It is unlikely that we shall see a real full-scale biography of Chicherin or significant new interpretations of early Soviet foreign policy until and unless *glasnost* extends much further into Soviet archives. This book owes much more to the work of other Western scholars than to the papers of Chicherin himself.

ROBERT C. WILLIAMS
Davidson College

ELLEN JONES and FRED W. GRUPP. *Modernization, Value Change, and Fertility in the Soviet Union*. (Soviet and East European Studies.) New York: Cambridge University Press. 1987. Pp. xiv, 420. \$44.50.

The aim of this volume is to provide a comprehensive treatment of how the processes of modernization and value change with respect to traditional family relations and sex roles have served to produce the transition from high to low fertility in the Russian empire and the Soviet Union since 1897. In certain respects the book is very impressive; in others it is disconcerting.

The first part of the book treats the patterns of social and demographic modernization in the Russian empire and the Soviet Union in the period from 1897 to 1960. The second deals with the social correlates of fertility variation by nationality and by geographic area in the Soviet Union in the contemporary period. A third part deals with how social policy has changed family values, which in turn has reduced fertility. The fourth part deals with how explicit demographic policy has been used in an attempt to increase Soviet fertility. In the final part of the book, it is shown how independent variables relating to an earlier period (for example, 1970) can successfully predict fertility at a later date (for example, 1979).

The most important contribution of this book, particularly for scholars seriously interested in the study of Soviet fertility, is its detailed exposition of the findings of the numerous studies conducted in recent years by Soviet demographers concerning the factors affecting actual and desired fertility. A related contribution, perhaps more important to the Soviet scholar whose major interest is not in the study of fertility, is the comprehensive presentation of major data compiled from Soviet vital statistics and from the censuses of 1897, 1926, 1939, 1959, and 1970. (Unfortunately, so few data have been released from the 1979 census to date that they are almost worthless from the standpoint of studying fertility.) Finally, the very comprehensive discussion of recent Soviet pronatalist policy and the debates among Soviet demographers as to what policy ought to be should also prove very useful to all Soviet scholars whether or not their prime area of concern is the study of fertility.

What I find disconcerting about this book is the authors' failure to set forth, in other than a cursory manner, what other Western scholars have had to say about fertility in the Soviet Union. Furthermore, almost all that they do report about such work is excessively disparaging.

Ellen Jones and Fred W. Grupp state that their purpose is to test three hypotheses: the first relating modernization to natality decrease, the second relating modernization to decrease in the strength of patriarchal family values mediated by a cultural filter, and the third relating decline in patriarchal family values to fertility decrease. The term "hypothesis" implies that we have only tentative knowledge. In reality all three of these so-called hypotheses are established paradigms accepted by all demographers. Moreover, Western scholars who have previously studied Soviet fertility have indeed long since confirmed each of them. But, because the authors pay so little attention to this previous work, many readers will get the false impression that up to now Western scholars have paid no attention to the topics treated here.

Furthermore, it is very irritating to a reviewer to see his own work misunderstood. The authors report on page 92, "Our findings are difficult to reconcile with Heer's." In reality my findings were identical to theirs. However, they misread my unstandardized multiple regression coefficient to be a zero-order correlation.

Because the authors have been inattentive to the work of other Western scholars, they fail to consider the very important problem of why the desired fertility of the Islamic nationalities of Soviet Central Asia is higher than that existing in the Islamic nations outside the Soviet Union despite the fact that the level of modernization among the Soviet Islamic nationalities is so much

higher. This puzzle is at the cutting edge of the research conducted both by Western and Soviet scholars and is obviously of major importance for the future ethnic composition of the Soviet Union.

DAVID M. HEER

University of Southern California

NEAR EAST

JOHN C. WILKINSON. *The Imamate Tradition of Oman*. (Cambridge Middle East Library.) New York: Cambridge University Press. 1987. Pp. xii, 415. \$59.50.

Before becoming an Oxford don, John C. Wilkinson worked in Oman's oil industry and came to know the country intimately. Initially, he conceived this work to be merely a narrative of the recent history of southeast Arabia showing how competition between two international oil conglomerates, the British-dominated Iraq Petroleum Company group and the Arabian American Oil Company, fanned a simmering rivalry for political dominance within the region after World War II. Although this confrontation was generally labeled the "Buraimi Dispute," by the 1950s it actually centered on control over Oman. Its most violent manifestation was the discord that pitted the British-backed sultan, who ruled Oman's coast, against the Ibāḍī Imam, who prevailed in its interior and was the rather reluctant object of Saudi Arabia's support.

To Wilkinson the key to understanding these events was knowing the nature of Oman's Ibāḍī Imamate and the tribal society on which it was founded. Eventually, his uncomplicated original plan evolved into an organizationally complex, profoundly thoughtful volume that analyzes two millennia of Omani history. It expertly links the underlying geographic, ecological, and socioeconomic factors that historically have conditioned human existence in Oman to long-term intellectual, religious, and ideological movements, as well as to the contemporary political events that were Wilkinson's initial concern. Moreover, he relates all this to Omani interactions with other parts of Arabia, Iran, India, and, especially, East Africa.

The book presents the Imamate as a religious expression of Oman's tribal system. It never developed a permanent hierarchy of administrative officials, its effectiveness depended on the personal ability of the Imam, and its authority was exercised through the cooperation of major local leaders scattered across the country. As a political institution it seldom functioned as more than a "semi-state," and, for twelve hundred years, a legitimate government in Oman was perceived as

one in which the Imam was not a law giver but a law enforcer whose actions were guided by the major religious leaders who interpreted Islamic law according to Ibāḍī tradition. Wilkinson sees Oman's history until the 1950s as a cyclical succession of attempts to realize this ideal in which any attempts to centralize authority always stimulated a centrifugal reaction. Most recently, the cycle was exemplified by the Ibāḍī *nahda* (renaissance) of the nineteenth and early twentieth centuries, which eventually was overwhelmed in the 1950s and 1960s. His interpretation provides plausible balance to the several sultanate-oriented explanations of modern Omani history that have appeared recently.

This is not an easy book to read—perhaps because it deals with complex concepts little understood even by specialists. Also, I have trouble accepting Wilkinson's conclusion that the "Imamate tradition" will not play a role in Oman's future. But these are quibbles. Wilkinson has combined his intimate familiarity with the hitherto little-known corpus of Omani writings and his firsthand knowledge of Oman itself with analytical skill and verve to produce a pioneering and seminal study.

ROBERT G. LANDEN

Virginia Polytechnic Institute and State University

HABIB LAJJEVARDI. *Labor Unions and Autocracy in Iran*. (Contemporary Issues in the Middle East.) Syracuse, N.Y.: Syracuse University Press. 1985. Pp. xviii, 328. \$29.95.

Despite its title, Habib Lajjevardi's book is a very good study not so much of the interrelationship between labor unions and autocracy but of the vital role played by foreign powers, essentially Great Britain, Russia, and, later, the United States, in the formation, rise, and fall of organized labor movements in Iran in the twentieth century. It provides ample evidence, based on British and American archival materials, for the author's argument that, while the Russian revolution "molded the character of Iran's labor movement and increased the pace in which it developed" (p. 4), Britain regarded it as a mere instrument of Soviet expansionist designs detrimental to its own interests in the region, a view the United States in turn came to share.

In eight interesting chapters Lajjevardi reveals the close collaboration that existed between Iranian trade unions, the Communist, or Tudeh, party, and the Soviet Union and documents British manipulation of the Ministry of Labor and its regulations. He depicts the period of successful

mobilization of Iranian workers by Communist-led trade unions and the gains achieved through strikes, work stoppages, and collective bargaining with employers as corresponding to periods of cooperation between the Western powers and the Soviet Union in Iran. Thus, for instance, the war period 1941–44 of Anglo-Russian alliance witnessed the reemergence of constitutional government in Iran and the formation of the Tudeh party and its affiliated Trade Union of the Workers of Iran. Though active chiefly in Tehran and Isfahan, both organizations chose to maintain law and order among the workers in Russian-occupied Azerbaijan and practiced a policy of nonintervention in British-occupied Khuzistan. The years 1944–46 were a period of confrontation as the tide of war receded and the leftist-dominated labor movement, encountering aggressive hostility on the part of wealthy employers, shifted from a reformist to a militant stance. Thus, independent trade unions were brought together in a confederation, the Central United Council of the Trade Union of Workers and Toilers of Iran (CUC).

The confrontation between the CUC and the state involved not only labor disputes but also political issues. Lajjevardi shows how, in response to the central government's rejection of the Russian request for oil concessions in the northern province, the Tudeh party and the CUC staged mass demonstrations and strikes in most major industrial cities in the fall of 1944. Tehran's reaction was, more often than not, dictated by international political considerations. In spring 1946, while Britain was already withdrawing its troops from the south and pressing for Russian evacuation from the north, Prime Minister Ahmad Qavam adopted conciliatory gestures toward the Tudeh party and the CUC. A comprehensive labor law was introduced for the first time in Iran, recognizing in principle the collective bargaining power of the unions and redressing some of labor's grievances. Simultaneously, however, and on British advice, a newly formed Ministry of Labor set up new regulations to ensure government control over the unions. Conversely, following the Russian evacuation of Azerbaijan, and when British and American representatives in the region increasingly viewed the trade unions as Soviet tools, covert and overt actions were undertaken to discredit and eventually destroy the power of the trade unions. Government-sponsored unions were allowed to emerge that made a mockery of the very idea of trade unionism.

The last two chapters and the epilogue, covering the period 1953 through the revolution of 1978–79, are less satisfactory. They are not as well documented; the far-reaching reforms of the 1960s and early 1970s are summarily introduced

and inadequately assessed. Workers' participation in the revolution is noted but not analyzed.

Ladjevardi has nonetheless demonstrated convincingly the futility of trade unionism and the powerlessness of capable labor leaders to form independent organizations in a country where not only autocratic regimes but also, more important, foreign powers are determined to stifle any movement they deem detrimental to their own interests.

MANGOL BAYAT
Newton, Massachusetts

AFRICA

ROBERT W. JULY. *An African Voice: The Role of the Humanities in African Independence*. Durham: Duke University Press. 1987. Pp. xii, 270. Cloth \$22.50, paper \$10.95.

The "African voice" in this book is that of intellectuals and artists who took part in the fight against Western cultural imperialism. Trained by expatriate teachers, they, nevertheless, rejected the wholesale adoption of foreign ideas and values at the expense of those rooted in the indigenous past. In the years following World War II, as African countries one by one gained political independence, these educated spokesmen and spokeswomen discussed, planned, and wrote about the revival of traditional cultural identities that, they believed, was essential to the intellectual decolonization of the continent.

The book is divided into four sections. The starting point is the idea of *Négritude*, those shared aspects of personality and culture that, it was argued, united all black people and were a key to the success of the liberation struggle. The Paris-based review *Présence Africaine* and the conferences of writers and intellectuals that it organized in the 1950s provided a forum for a lively debate in which Marxists, black Americans, West Indians, and others participated. In a second section Robert W. July discusses the contribution of the visual arts, of the theater, of musicians and dance companies, and of novelists to African cultural independence. He shows how traditional art forms that persisted in the colonial period, in spite of prohibitions by European administrators, missionaries, and teachers, were the basis for a postindependence renaissance. In the late colonial period, those trained in Western schools were an organizing force in setting up companies and institutes for the performance and study of indigenous music, drama, and art. The third section of the book turns to the role of education in encouraging the revival of traditional cultural forms, to

African historians who pioneered the writing of an indigenous history in which oral tradition was a principal source, and to the new universities and their conflicts with national governments. In a final section July highlights a continuing debate among African intellectuals about the relevance of traditional values and institutions in the contemporary world. Some advocate the abandonment of notions such as *Négritude* as a comfortable but unprogressive romanticism that is unhelpful in solving contemporary political strife and economic weakness. Others who recognize the pervasiveness of Western institutions and the necessity of Western technology maintain, nevertheless, that cultural autonomy is essential to African political and economic independence.

This is a lively discussion made all the more so by the fact that July has been visiting or working in Africa for some three decades. He draws on personal observations, diaries, interviews, and correspondence, as well as contemporary accounts by others. One regrets the rather narrow focus of the book on West Africa. A large part of the discussion is given over to personalities and developments in Ghana and Nigeria, with which the author is particularly familiar. It would certainly have been useful to have considered the working out of these themes in the South African context as well. The book raises important issues about the ambiguous role of the educated elite in the colonial period and the years immediately following independence. Their relationship with grass-roots popular culture deserves to be explored further.

PHYLLIS M. MARTIN
Indiana University

RICHARD L. ROBERTS. *Warriors, Merchants, and Slaves: The State and the Economy in the Middle Niger Valley, 1700–1914*. Stanford: Stanford University Press. 1987. Pp. xii, 293. \$39.50.

This book is about the social and economic dynamics of power, domination, and exploitation. According to Richard L. Roberts, the salient theme of the economic history of the Ségou region between 1700 and 1914 lay in the constant struggle to strike a viable balance between the predatory inclinations of a succession of warrior-rulers and the productive capacity of the peoples they sought to dominate. The key element in this equation was the control of labor, particularly in the form of slaves.

Slavery was at the heart of the economic activities that most interest Roberts: the fishing and transport services of the riverine Somono and the commercial and plantation enterprises of the Maraka. The Somono originated as slaves of the

Bambara rulers, who directed them toward their riverine enterprises and subsequently swelled their numbers with war captives. Similarly, the Maraka populated their plantations with the slaves taken by Bambara warriors. Of course, free elements of the population were also economically productive: the mass of Bambara farmers, the cattle-herding Fulbe, and the various craft groups such as blacksmiths. But slaves were numerous; estimates for the end of the nineteenth century exceed 50 percent of the population.

These slaves were produced by the predatory ruling classes, who during the two centuries covered by this study constituted themselves in three successive states: the Bambara dynasties of the Coulibaly and Ngolossi, the Futanke forces led by al-Hajj Umar Tal, and the imperial armies of the French. Each of these polities is evaluated in terms of its role in developing the economic potential of the society. The Bambara, in spite of their hard-drinking and hard-fighting ideology, receive a fairly good assessment in economic terms, primarily because of their policies of channeling a labor force in the form of surplus slaves into the Maraka and Somono communities, which enabled these groups to consolidate and expand their productive activities. The Umarians, by contrast, performed poorly as economic policy makers who tended to humiliate and exploit the populations they dominated. Of course the differing economic "policies" of these two states were a function of their security and the extent of their direct control. The Umarians, who were constantly under threat of rebellion, never managed to establish any sort of stable administration, and one questions to what extent their domination deserves to be described as a state.

The French record is paradoxical. The only one of these ruling groups completely to monopolize warfare, they oversaw, in spite of their own cautious policies, the self-liberation of some twenty thousand Maraka slaves, who simply walked away from their masters. Of course, French economic policy was based on wage labor, but the French hesitated to pursue a forceful policy of emancipation for fear of social and political disruption. Nonetheless, the slaves clearly perceived how the new political order could work to their advantage, and they liberated themselves. Political conditions had changed, but it can be argued that these slaves freed themselves in spite of the state.

This event, which constitutes the concluding chapter of Roberts's book, demonstrates one of the central themes of his analysis: the evaluation of economic development in relationship to the "field of force" set in motion by state power. One can speak of "policies" in these states, but these were transitory; the major concern of the domi-

nating groups of warriors was to remain in power. They were constantly struggling among themselves for internal dominance, and their major source of income was predation. The real initiative for economic development originated outside the seats of state power, within groups such as the Somono and Maraka, whose success depended on how well they managed to operate within the "field of force" that emanated from the state. This was a kind of juggling act in which one sought to maintain a friendly but distant relationship to the state, allowing maximum freedom and minimum interference.

Slaves were, in a sense, a by-product of the predatory warrior state. The slave plantations of the Maraka were developed less by any explicit state policy than by the social and political conditions generated by the self-serving interests of the rulers. Warriors produced slaves; the Maraka plantations absorbed them. When these same plantations became the target of raids under the Umarians, the Maraka did not have the means to protect themselves. Furthermore, the slaves remained on these plantations not because the Maraka themselves possessed the physical power to keep them there but because political conditions within the society left the slaves no option. When conditions changed under French authority, the slaves seized the initiative and walked off the plantations.

LOUIS BRENNER

*School of Oriental and African Studies
University of London*

JAMES WESLEY SMITH. *Sojourners in Search of Freedom: The Settlement of Liberia by Black Americans*. Lanham, Md.: University Press of America. 1987. Pp. xiii, 228. \$24.75.

This most informative book about Liberia traces the history of the republic from its origins in the 1820s to 1847 when it became an independent nation. James Wesley Smith covers familiar ground in his description of the major problems confronting the settlers on their arrival in West Africa. According to Smith, initial attempts by the settlers to locate close to the British colony of Sierra Leone were thwarted by British officials who disapproved of the establishment of a nearby independent black settlement. Thereafter, settler attempts to seek refuge at Bonthe Island, near Sierra Leone, proved even more disastrous as many of them were killed by an inhospitable climate and malaria.

Subsequent efforts to resettle blacks on the west coast of Africa proved more successful. In 1821 the first agreement for the purchase of land for

the American settlers was reached between representatives of the American Colonization Society and the African chiefs of the Cape Mesurado area. Similar agreements followed that allowed for the steady expansion of the settlement. State-sponsored colonialization societies also participated in the establishment of the republic through the purchase of land from local West African authorities. The various settler groups were united by their common origin as freed slaves from North America and determination to establish themselves on African soil. Consequently, their growing presence was increasingly opposed by the indigenous Africans, which in turn simply deepened the settlers' resolve to stand firm.

The most illuminating part of the book is the author's meticulous and comprehensive documentation of the various settler groups. Each arriving group is carefully identified, including its place of origin in the United States, point of arrival in Liberia, and adjustment problems. Although the history of the early settlers has been previously analyzed by Tom W. Shick and P. J. Staudenraus, Smith's account of their experiences is readable and easily comprehensible. The intrigues and the numerous struggles between the white agents and black settlers for control of the republic are well documented and presented in an unbiased manner.

Nevertheless, a few minor gaps require further clarification. For example it would be interesting to know the author's views on the legality of the land agreements signed between the settlers and the Africans. Some writers such as G. E. Saigbe Boley and Staudenraus contend that the agreements were highly questionable because the Africans did not understand the details and legal ramifications of the treaties or land agreements. Also, Smith seems to downplay the use of force by settlers to secure the signatures of the African tribal authorities as further evidence of the illegality of these agreements. Since land in Africa is generally treated as communal property entrusted to the chiefs by their people for safekeeping, might not the Liberian chiefs have thought they were leasing rather than selling the land to the settlers?

Smith could have elaborated on the circumstances whereby former slave settlers came to exploit the indigenous people. Indeed, settler discrimination against the Africans became rampant, and many of them were denied the vote and restricted in their heretofore free movement. Settlers often referred to them as "uncivilized" and subjected Africans to indentured servitude. Such abuse of the African population by the settlers has been very well documented in previous works.

Smith characterizes initial U.S. policy in Liberia

as "lukewarm" and accuses State Department officials of indirectly contributing to the problems of the settlers by refusing to openly support the black republic. This is a rather simplistic conclusion. First, American policy in Liberia was influenced by a number of factors, including the question of slavery, which was a major divisive issue in the United States at the time of Liberia's establishment. Therefore, the State Department had to be very cautious in its approach to overseas black settlements. Besides, Africa was considered to be in Europe's sphere of influence. Aggressive intervention in Africa by the United States at the same time that it was enunciating the Monroe Doctrine would have led to serious opposition from France and Great Britain, whose colonies of the Ivory Coast and Sierra Leone were adjacent to the Liberian settlement. Further, the United States lacked a formidable navy, and, given the climate and course of events leading to the Civil War, the possibility of the United States entering the colonizing race was out of the question. The United States accordingly adopted the quiet and sensible approach of cooperating with the Europeans in Africa. American officials appealed to the French and British governments to be reasonable in their dealings with the black republic. Eventually, this policy proved successful.

The above points notwithstanding, Smith's book is based on sound and extensive research, and this makes it a valuable addition to the growing literature on Liberia.

HASSAN SISAY
California State University,
Chico

MEGAN VAUGHAN. *The Story of an African Famine: Gender and Famine in Twentieth-Century Malawi*. New York: Cambridge University Press. 1987. Pp. viii, 181.

Megan Vaughan has provided a thorough and informative discussion of the historical context for a major famine in central African history, the famine of 1949 in colonial Malawi. She evaluates three types of explanation for famine; these theories were used to identify the causes of the famine of 1949 and are found in discussions of present famines. In addition she explores the gender dimension of "who starved and why." The result is a complex methodology that yields a complex analysis.

The first of the three theories is neo-Malthusian. Colonial experts were convinced that African population was outstripping arable land. In Vaughan's view they made several errors. Their aggregate figures were meaningless, given the

variability of population density and land fertility. They assumed that the model African population was self-sufficient in food production, despite the fact that for more than a century families had procured food through exchange as well as farming. Moreover, they ignored the political-economic context, particularly land alienation by European individuals and enterprises, which significantly affected the amount of land available for African cultivation and the conditions under which that cultivation occurred.

Although the neo-Malthusian theory was the most prominent, other explanations were put forward. Some Europeans claimed that maize production had declined because high tobacco prices led African farmers to substitute the latter. Although Vaughan credits this with some validity at the national level, she finds little evidence that individual farmers in fact made this substitution. The variability of land holdings and fertility meant that some African producers, as tenants on European estates, did not themselves make that decision and that tobacco producers were likely to be successful food producers as well.

The third theory stressed the inappropriate intervention of the colonial government in food marketing, for example, through the Maize Control Board. Although she also credits this theory with some validity, Vaughan asserts that this view assumes a greater impact of maize legislation than appears to have been the case.

Her complex explanation stresses the interconnection of population migration, the commercialization of agriculture, government interventions in farming and marketing, and increasing wage labor.

Creatively employing an analysis of women's maize-pounding songs, Vaughan explores the extent to which gender determined who starved and why. Along with the young and the aged of both sexes, women suffered. The most vulnerable were married women whose husbands were absent: the government assumed them to be cared for. Least vulnerable were men employed in the formal sector and their dependents: the government targeted them for relief schemes to quiet the most vocal African critics of the government and their European employers. The women's pounding songs reveal women's discomfort with their increased dependence on men in uncertain times and with the high rate of desertion in the face of calamity. The famine strained the matrilineal fabric of this society.

The research base and organization of this study are commendable. However, the author's discussion of gender would have benefited from an explication of the significance and rise of gender as a category of analysis comparable to her

excellent discussion of the theoretical frameworks used to study famines.

MARGARET STROBEL
University of Illinois,
Chicago

JEFFREY BUTLER *et al.*, editors. *Democratic Liberalism in South Africa: Its History and Prospect*. Middletown, Conn.: Wesleyan University Press or David Philip, Cape Town; distributed by Harper and Row, Scranton, Pa. 1987. Pp. xiv, 426. Cloth \$35.00, paper \$14.95.

White liberals such as Helen Suzman and the late Alan Paton have played a visible part in contemporary South African political life and may be seen as part of a tradition of liberalism that goes back to the nineteenth century at the Cape Colony. This new book raises the question as to whether today liberals are trapped between the extremes of Right and Left and thus doomed to irrelevance, especially at a time of revolutionary choice. Liberals have long been despised by Afrikaner nationalists, who see them as "kaffer boeties," or lovers of blacks, and since the 1950s they have also come under fire from Marxists and many black leaders, especially black consciousness radicals.

The book is the result of a conference held at Houw Hoek near Cape Town in 1986 in which the participants were all scholars who had done intensive research on the liberal tradition in South Africa and included Frederik Van Zyl Slabbert, former leader of the Progressive Federal party, a number of U.S., Canadian, and South African academics, and a newspaper editor. In their introduction Jeffrey Butler, Richard Elphick, and David Welsh characterize the participants as "democratic liberals" committed not only to such liberal values as the rule of law and a free press but also to free and open elections and to a "black preponderance in government." Their hope was that both the conference and the subsequent volume would enhance the long-term chances of a democratic liberal outcome of the South African crisis. The editors, however, were "to our great regret" unable to obtain papers from black scholars. This is an absence that perhaps more than anything else epitomizes the isolated status of white liberals.

The first part of the book is an excellent survey of the liberal tradition in South Africa, and parts 2 and 3 focus on liberal interpretations of South African history and contemporary political issues. It is in this segment of the book that the real challenges to liberal thought become evident. Today, the main theoretical and practical challenges to liberal scholarship come from the Left. Marxist

critics challenge the historians, anthropologists, and economists who formulated the liberal interpretations of South Africa's institutions and history. For example Marxist scholars are highly critical of liberal historians who interpreted South Africa's racial order in terms of Afrikaner Calvinism and the ignorance and isolation of Afrikaners on the frontier or who saw British imperialism as an enlightened counterforce to these influences. Liberal historians paid little attention to class, which has now become a central part of historical analysis, emphasizing instead the importance of race. In challenging the Marxist approach, the editors maintain that "Part of the power of the Marxist analysis of South Africa derives from its ability to treat all aspects of South African reality within the holistic vision of the political economy. To the non-Marxist the danger of the Marxist vision is that one pattern, the 'capitalist relations of production' or the class structure, tends constantly to be brought into relief, so that all of the distressing features of South African society—racism, human exploitation, inequality, poverty—are repeatedly attributed to capitalism. This attribution, more often than not, derives more from the model itself than from argumentation or evidence" (p. 9). This standard liberal critique of Marxist thought tends to trivialize the real impact Marxists have had on scholarship and thought. A clear difference then exists between liberal and Marxist scholars in interpretation and ideology, and the main ideological contender against liberalism is no longer Afrikaner nationalism but Marxism.

In many ways, white liberals are still caught in the middle. They reject the uncompromising views of the Right and, indeed, of the ruling Nationalist party. Although sympathetic to African aspirations and to the idea of a nonracial society, they are unwilling to commit themselves fully to the African nationalist movements or to an economic reordering of South Africa. Because of their ties to the capitalist structure, most of the authors agree that capitalism would produce the wealth needed for a just society in South Africa and limit the ability of government to trample on human rights.

This does not mean that the liberals cease to be relevant. Parts 4 and 5 deal with the key institutions of democratic liberalism in South Africa today—law, press, and education—and with the relevance of democratic liberalism in the current crisis. The overemphasis on the scholarly debate by the contributors tends to overshadow the continuing significance of the liberal tradition for political issues. Perhaps the most valuable contribution of white liberals has been their ability to keep some dialogue open with black South Africans. This became dramatically apparent in May

1987 when sixty-one Afrikaners met in Dakar, Senegal, with seventeen representatives of the African National Congress. They defied the South African government by setting up this meeting but at the same time opened the way for further dialogue between black and white South Africans.

This is an interesting and well-organized book. Its importance lies in its analysis of those who, despite their ambiguous political position, have kept lines of communication open between races and who have been able to inject an alternative dimension into the political process.

PATRICK O'MEARA
Indiana University

ASIA AND THE EAST

LI XUEQIN. *Eastern Zhou and Qin Civilizations*. Translated by K. C. CHANG. (Early Chinese Civilizations Series.) New Haven: Yale University Press. 1985. Pp. xvi, 527. \$55.00.

The Eastern Zhou period (770–221 B.C.) was an age of great transformations in ancient China. Fundamental changes took place in Chinese society, economy, politics, military, and culture during that era, and as a result China moved from the stage of feudal order to that of centralized bureaucratic empire, which marked in conventional terms the end of classical or ancient China and the beginning of imperial China. Li Xueqin's *Eastern Zhou and Qin Civilizations* is an extensive study of those changes and the emergence of the first imperial dynasty, the Qin (221–207 B.C.), that followed that period of great transformations. Its unique value is its use of modern archaeological data as its main primary sources. In the first part of the book, Li describes the important archaeological sites of Eastern Zhou and Qin, providing a historical background for each with both archaeological and textual data. In the second part he traces the development of eleven categories of artifacts and writings throughout the period. This is a monumental work in every way and should be considered an essential reference for all historians, archaeologists, and art historians in the China field as well as for anyone who is interested in ancient Chinese civilization.

The Chinese text of the book is entitled *Dongzhou yu Qindai wenming* (1984) and is arranged in a format slightly different from the English version, the chapter "Social Classes," for example, being in part 1 in the Chinese text but in part 2 in the English version. A long bibliography and some new materials have also been added in the English version. K. C. Chang has not only rendered an accurate and highly readable English version of

the Chinese text but also turned it into a better reading for both specialists and general scholars.

Li is generally recognized as the foremost authority in China on the archaeology of the Eastern Zhou and Qin periods, and his book clearly confirms this reputation in its unusual breadth of scholarship and unique command of sources. My highest admiration for his scholarship notwithstanding, a number of points in Li's book are still open to question and need further discussion. I shall give three examples here. First, Li states: "In the year 206 B.C., Emperor Gaozu of Han was enthroned, and a new, important dynasty in Chinese history, the Han dynasty, came into being" (p. 9). This is incorrect. The Han dynasty did not begin in 206 B.C.; it came into being in 202 B.C. In 206 B.C. Liu Bang, the later Gaozu, was made only the king (*wang*) of Han, and his kingdom was only one of the nineteen such states that had been established in 206 B.C. after the fall of Qin. The four years from 206 to 202 B.C. were a critical period in early Chinese history. During that time China again had fallen into disunion and suffered from continuous bloody wars among the contending states; it had then returned to the divided world of the Eastern Zhou times before the Qin unification. Hundreds of thousands of people died, and millions more were dislocated during that period of destruction. The Han dynasty did not exist in this period, and the years 206–202 B.C. were clearly a separate historical period, not a part of the Han dynasty, which began in the spring of 202 B.C. when Liu Bang assumed the title of "emperor" (*huangdi*). Second, Li's treatment of the Great Wall and its cultural significance is not up to date. For instance the new archaeological finds along the Great Wall in Gansu Province have already produced new views about the physical structure and cultural implications of the Great Wall, but they have been overlooked in Li's study. Third, although Li has cited an unusual number of primary sources and secondary works in Chinese and Japanese, some of the most important have not been mentioned. These include, among others, the works of such well-known scholars as Li Zongtong, Lü Simian, Cheng Faren, Nishijima Sadao, Kurihara Tomonobu, and Satō Taketoshi. Takezoe Kōkō's classic *Saden kaisen* should also be cited together with Yang Bojun's work (p. 11), for it is still one of the most comprehensive and convenient editions of the *Zuo Zhuan*.

CHUN-SHU CHANG
University of Michigan

JOSEPH NEEDHAM. *Science and Civilization in China*. Volume 5. *Chemistry and Chemical Technology*; part 7, *Military Technology: The Gunpowder Epic*. Assisted

by HO PING-YÜ *et al.* New York: Cambridge University Press. 1986. Pp. xxxiii, 703. \$99.50.

This authoritative volume outlines the Chinese origins of gunpowder, guns, and rockets and then traces both their transmission to the West and their eventual reintroduction to China. The scholarly achievement is impressive, particularly when one considers the linguistic challenges posed by the sources, which often use a single term to describe quite different objects (trebuchets and cannons, for example). Overcoming such difficulties, however, the authors provide precise descriptions and good dates for the Chinese discoveries.

The first steps came in the ninth century, when Taoist alchemists discovered the explosive properties of saltpeter, sulphur, and carbon in combination. By the tenth century the mixture was being placed in roughly spherical bombs and mines for military use. But the full possibilities of this *huo yao*, or gunpowder, came to be understood only a few decades later, when it began to be placed in bamboo or other cylindrical casings. Such explosive-filled tubes were initially held on the ends of spears and used as flamethrowers in defense of cities. But before long they were being allowed to propel themselves, as rockets, or used to expel arrows or other projectiles. Military rockets appeared in the late twelfth century and were used in the early Ming against the forces of the prince of Yen (though without success). The true gun (which Joseph Needham defines as having a metal barrel, high-nitrate powder, and a projectile that fully occludes the muzzle) appeared toward the end of the thirteenth century, and the first artillery piece seems to date from sometime in the fourteenth century.

These discoveries reached the West quite rapidly and fully developed (a bombard is shown in a manuscript of 1327). Needham believes that the fireworks Roger Bacon knew came from China and suggests that Bacon's interest in the prolongation of life (a classic Taoist concern) also reflected Chinese influence. Knowledge of guns and cannons appears to have been transmitted through Russia.

But, however the process began, firearms technology developed rapidly outside of China, and after a few centuries it began to flow back. The contributions of the Jesuits and others are presented in detail, and the role of Vietnam (which the Ming history seems to make the source of firearms technology in general) is clarified. Some stimulating conjectures are offered as well: the better muskets of the sixteenth century, for example, may have come from Turkey. Furthermore, Needham shows that even at the time when the

West was beginning to draw well ahead in military technology, the Chinese were still innovating. Among late seventeenth-century inventions were a kind of rapid-firing gun and a shell-firing cannon, both of which contributed to K'ang-hsi's victories against nomadic adversaries.

The changes in military technology documented here must have had some impact on Chinese history, but being specific is difficult. Good accounts of battles are rare, and it is often easier to document inventions than their uses. But gunpowder clearly contributed to the tremendous size of both the Ming and Ch'ing empires. According to Needham, the attention the Ming founder Chu Yuan-chang paid to firearms technology may have contributed to his success against the Mongols; certainly, the dynasty always maintained a special secret office specializing in "magically efficacious" weapons. Ch'ing use of firepower, which the volume documents more thoroughly, clearly helped hold that empire together.

This volume necessarily treats such broad questions only in passing. But, by providing a firm historical baseline of the development of firearms technology in China, it prepares the way for others to show either how it changed that society or why it did not.

ARTHUR WALDRON
Princeton University

PETER C. PERDUE. *Exhausting the Earth: State and Peasant in Hunan, 1500-1850*. (Harvard East Asian Monographs, number 130.) Cambridge, Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1987. Pp. xvii, 331.

Peter C. Perdue sets the context for this monograph by describing two views of traditional Chinese government: Oriental despotism versus the limited state. The first school begins with Montesquieu's claim that absolutism suppressed China's development. Their opponents, from Quesnay on, see a state resting lightly on its subjects and tolerating, if not promoting, economic change. Perdue aims to illuminate this issue by examining the workings of the bureaucracy in a specific region (Hunan province) and giving special attention to a period (the eighteenth century) when traditional institutions were at their healthiest.

The complexity he finds seems to overload his taxonomy. It is better, perhaps, to add a dimension by subdividing each model into favorable and unfavorable variants: despotism that stifles development; despotism that positively mobilizes resources; a limited state that invites disarray; a limited state that allows the market and private

incentives to work. The scholars Perdue discusses cluster around either the first category (Karl Wittfogel, for example, who stresses retardation) or the fourth category (for example, Ramon Myers, who sees the traditional state more positively).

But Perdue's Ming and Qing Hunan, although experiencing some dysfunctional despotism, such as punitive taxes in the Ming (the first category), saw state intervention more often taking a constructive form such as land reclamation, water control, and repopulation following periods of disorder (the second category). State-assisted economic recovery laid the base for private activity, which revenue-starved local administrators could, at best, guide and regulate. Here is a modicum of "good" laissez faire (the fourth category). Finally, eighteenth-century commercialization and population explosion destroyed the bureaucracy's ability to control ecological degradation and social conflict. This led to disarray (the third category). Thus, what Perdue finds is not an unchanging state-society nexus but a cycle.

The monograph highlights some important distinctions. There are regional and intraregional geographical peculiarities: for example, Hunan's uplands (slash-and-burn farming, need for reservoirs, problems of erosion and soil exhaustion) differed from lowland Hunan (rice agriculture, networks of diked polders, problems of flooding and silting). There were regional differences in the customs governing land tenure and landlord-tenant relationships and divergent chronologies of change—the lower Yangzi region having experienced specialization and commercialization centuries before Hunan.

The book contains valuable discussions of the differential forces at work on local, provincial, and central officials. Those at the base of the hierarchy had to cope with an inadequate land registration system and tax base, with social conflict brought on by immigration and social inequalities, and with litigation over land disputes, tenant relationships, and so on. But these same officials were susceptible to the corrupting pressures of the local gentry and government underlings. By contrast provincial officials could take a larger view and were often the ones who recognized the ecological implications of unchecked private dike building and the problems posed by overpopulation.

The monograph does have shortcomings. The argumentation is often needlessly equivocal, as in the following: by 1800 Hunan agriculture was flourishing but only temporarily; the government had played an important part but not decisively important; the government could not resist general trends but was not completely passive; it exhibited "impressive strengths" but also "fatal weaknesses" (pp. 134-35). Surely, this could be

improved on. The author also sometimes generalizes from scanty evidence. He supports the case for widespread, extended famine with references from a single county (pp. 130–31). He highlights exemplary officials without providing a basis for judging how representative they might have been (for example, Zhao Shengqiao [pp. 80–81]).

These flaws, however, cannot overshadow the many merits of this enlightening and beautifully produced monograph.

CRAIG DIETRICH
University of Southern Maine

STEVEN I. LEVINE. *Anvil of Victory: The Communist Revolution in Manchuria, 1945–1948*. (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1987. Pp. xi, 314. \$35.00.

Steven I. Levine's study of the struggle between the Communists and Nationalists over China's northeast in the years from 1945 to 1948 is a significant and provocative addition to the literature that attempts to explain how the Chinese Communists came to power. By focusing attention on the northeast, a region with a very different history from intramural China, his study makes it possible to examine old issues in this debate from new angles.

Levine argues that we have been seeking the roots of victory within a field that is much too narrow. Because our attention has been captivated by the startling growth of Communist forces from a small group of some twenty thousand survivors of the Long March in 1936 (only months before the Japanese invasion of intramural China in 1937) to almost a million men by the time of the Japanese surrender, we have concentrated our search on the Communist base areas created during World War II. But Levine insists that the answers thus dug up provide us with only a partial and perhaps even a misleading picture of how the Communists came to power. To be complete, our vision of the victory must also include the civil war in the northeast, a struggle that took place after the Japanese surrender and outside the Great Wall, in a region where there had been no wartime bases.

The inordinate significance that Jiang Jieshi (Chiang Kai-shek) and the Nationalists placed on the battle for the northeast is well known. The region, equal in size to France and West Germany and located at the same latitude, contained important modern industries, including heavy industries of much military significance, as well as considerable agricultural surpluses. For the Nationalists it also had another great advantage. The forces of

Manchukuo, the puppet state created by Japan after its seizure of the region in 1931, had crushed all resistance, Nationalist and Communist. Thus, in August 1945 it was the one place in the north that was entirely free of Communist influence.

Levine makes clear that the Communists, too, understood the region's strategic and symbolic value and committed an inordinate amount of men and material to the contest. In 1945 they sent to the northeast about one hundred thousand of their best troops (approximately one-sixth of all their regular forces), and some ten thousand cadres, including about one-fifth of the regular members of the Central Committee and close to one-third of its alternate members.

It was here, according to Levine, that the victor was decided. In the autumn of 1948, after a three-year contest, Communist armies destroyed the best of the Nationalists' military forces and did serious damage to the morale of those that remained. Thereafter, the victorious armies, now filled with the peasants of the northeast, broke through the Great Wall and captured Beijing (then Beiping) in January 1949. By October, when the People's Republic was proclaimed, they had fought their way to Guangzhou on the southern coast.

Through his description of this portion of the civil war, Levine hopes to put to rest the revolutionary romanticism that has characterized some earlier analyses of the Communist victory. The battle for the northeast was no guerrilla war fought by peasant partisans but a conventional war fought by regular armies. Nor did any peasant revolution simply well up from northeastern soil. The Communist party orchestrated a hasty land reform marred by a lack of personnel and an often unrestrained use of violence.

A number of Levine's statements are debatable and, indeed, deserve to be debated, and some need more complete consideration. But none of the debatable points is an essential part of the foundation that underlies his most important conclusion: that it was the land reform that provided the means, political and military, for the Communist success, even in this region of conventional warfare. Even though it was not their own idea and its implementation was fraught with problems, the farm families that benefited from the land reform supplied the Communist party with the local support that ultimately defeated the Nationalists.

LYNDA N. SHAFFER
Tufts University

ASHWANI SAITH, editor. *The Re-emergence of the Chinese Peasantry: Aspects of Rural Decollectivisation*.

New York: Croom Helm. 1987. Pp. ix, 277. \$57.50.

Between 1956 and 1978 the Chinese Communist Party (CCP) forcibly turned villages into collective organizations. The CCP's decision to decollectivize by allowing households freely to manage the land and farm capital given them by the collective unit in exchange for a fixed quota of crops was made in 1978 but only enforced for the entire country in 1982. These eight essays describe the momentous changes that have taken place between 1978 and 1984 with some comparisons for previous years.

The authors agree that, in regaining greater freedom to exchange and allocate their land, labor, and capital, rural families have greatly improved their livelihood and farming has become more productive. Rural poverty has even been eliminated in many areas. They also concur that more household specialization in farming, the elimination of many state price controls, the expansion of free markets, and the growth of rural industry and services (much of it privately owned and managed) have brought an unprecedented prosperity to the countryside.

Most of the authors blame the socialist policies of the years 1956–78 for the dismal economic performance of the rural economy. That collective farming system produced a "dichotomy between a growing rural professional elite on one side and a majority of 'dumb' peasants reduced to basic manual work on the other" (p. 19). CCP policies also forced rural collectives to be self-sufficient in food production and to develop their self-sufficient rural industries, discouraged services from expanding, and maintained high savings in order to increase spending for collective projects and social welfare. These socialist policies proved to be disastrous. Data presented to compare income and production between 1978 and 1984 and previous years tell that story very convincingly.

Collectivization discouraged innovation and hard work; it also encouraged an enormous misallocation of resources, which in turn wasted savings and physical capital. The same system also promoted little or no growth of factor productivity and made possible state pricing policies that exploited the rural people and prevented any substantial improvement in their real income and social welfare.

Some authors in this volume, however, worry that under the new household contract system income distribution between regions and households might become more unequal; many collective units seem unable to maintain their social capital because they have less control over household income; households also must be responsible for their social security when their members reach

old age as well as for educating their children; and the state's single-child-per-family policy still has not satisfactorily slowed the growth of population. These are some of the trade-off effects of the CCP's decision to decollectivize.

Editor Ashwani Saith and the authors see decollectivization as the reemergence of the peasantry. They do not quite know what to make of this new phenomenon. Why they prefer to use the pejorative term "peasantry" is not clear. "Peasantry" means an uneducated people of low social status tilling the soil as small landowners or as laborers. Before 1949 China's rural people were frugal, hard-working, and sophisticated farmers living in well-integrated town-market-village networks. According to Confucian standards of evaluation, farmers held a high social status. Why not refer to today's rural households that primarily farm under the new collective unit-household contract system as family farms instead of peasant holdings?

Most of the essays imply that had socialist China introduced the household contract system in 1954–56 instead of 1978 performance of the agrarian sector certainly would have been far superior between 1956 and 1978. Yet this study refrains from drawing that conclusion. Perhaps it is still early to determine whether the CCP will remain committed to the new policies it believed were necessary to correct the failed policy of collectivization.

But, as these essays appear to argue, never have the prospects appeared so favorable for China's rural people to use their great talents to improve their welfare and spiritual life.

RAMON H. MYERS
Hoover Institution on
War, Revolution, and Peace
Stanford, California

PETER LOWE. *Origins of the Korean War*. (Origins of Modern Wars.) New York: Longman. 1986. Pp. xvi, 237. \$12.95.

Diplomatic and military historians surely will applaud Harry Hearder's decision as general editor to include a volume on the Korean conflict in his *Origins of Modern Wars* series. After reading the finished product, they may wish Hearder had chosen someone else as the author. Peter Lowe's account lacks balance and coherence, suffering from an absence of both consistent analysis and a unifying theme. He contends "that the origins of the Korean War are best understood in the light of developments in the Korean peninsula, in China, Japan and in Europe" (p. x). After showing how Soviet-American military occupation in 1945 pro-

duced two Koreas "spurred on by burning zeal . . . to liquidate the other" (p. x). Lowe summarizes the evolution of the cold war before 1950 in Japan, China, and Europe—as much as one can in just over seventy pages. Placing the Korean conflict in its international context has value, but the author rarely demonstrates the direct connection between events outside Korea and the coming of the war.

Readers will have trouble deciding what this book adds of significance to existing knowledge of the Korean War. Attributing the conflict more to "muddle" than "malevolence," Lowe offers nothing new when he concludes that "Korea was a victim of her own murderous internal animosities and of the mutual suspicion and hatred of the superpowers" (p. 215). Worse still, the title is misleading, since less than half the book focuses on Korea itself prior to June 25, 1950. Lowe claims his "story of the origins of the Korean War . . . ends with China's entry" (p. 201), but his conclusion (another misnomer) covers in cursory fashion the truce talks, CIA involvement in the 1960 ouster of South Korean President Syngman Rhee, and idle speculation about possible reunification after the death of North Korean leader Kim Il Sung.

Since both author and publisher are British, it is not surprising that this book devotes inordinate attention to Britain's involvement. In particular Lowe spends much time discussing and defending British criticism of U.S. policy. Before the war London accurately assessed the American position in Korea as ambiguous and vacillating. Lowe states categorically that, rather than abandoning the Kuomintang, the United States "had already decided before June 25 that Taiwan must not fall to the communists" (p. 154). Once involved in the war, the United States could have averted China's entry "by the adoption of more sensitive, realistic policies" (p. 201), specifically, following Britain's lead and recognizing the People's Republic. Instead, a "phobia over communism" (p. 105) was responsible for "the sweeping nature of American policy-making with the tendency to overreact in unduly emotional terms" (p. 168).

There are other problems, including the consistent introduction of individuals without first names. Lowe typically relies on a single secondary work for background information. While paraphrasing and extensively quoting contemporary Soviet, Chinese, and North Korean speeches and propaganda literature, his use of U.S. archival material is highly selective. Ignoring contrary evidence, Lowe insists that Secretary of State Dean Acheson "and certain others" (p. 161) advocated the immediate commitment of U.S. ground forces to halt North Korea's invasion. Four maps appear inconveniently at the end of the text with a bare-

bones bibliography. Lowe does rely heavily on British archival records, often providing valuable new insights. Aside from this, the book has major flaws discouraging its use even in the classroom.

JAMES I. MATRAY

University of Southern California

PETER DENNIS. *Troubled Days of Peace: Mountbatten and South East Asia Command, 1945–46*. (War, Armed Forces, and Society.) New York: St. Martin's. 1987. Pp. xi, 270. \$37.50.

When the surrender of Japan brought a sudden end to World War II in August 1945, Japanese armies still controlled most of Southeast Asia. The task of reoccupying this huge area, freeing over one hundred twenty thousand prisoners of war and internees, disarming more than three-quarters of a million enemy troops and civilians and returning them to Japan, and restoring law and order fell to Admiral Lord Louis Mountbatten, supreme allied commander, Southeast Asia Command.

It was a difficult, if not impossible, mission. The million and a half square miles of Mountbatten's command included 128 million people, no effective civil government, and a number of rising nationalist movements opposed to the return of their former colonial masters. He lacked sufficient troops and shipping to meet his broad responsibilities, and Japanese forces were uncooperative and sometimes disruptive. A final obstacle was the inability of the British government to make up its mind about Southeast Asia policy, thus denying him meaningful guidance and support.

How Mountbatten met this challenge, how he succeeded in some of his tasks and failed in others, and how the events of 1945–46 laid the groundwork for subsequent developments in Southeast Asia are the themes of this useful and ambitious work. The book's title is somewhat misleading: its scope is limited to French Indochina and the Netherlands Indies, all but ignoring developments in Burma, Siam, Malaya, and Singapore. Nevertheless, the author has written a detailed and thoughtful account, sympathetic to Mountbatten's burdens and dilemmas yet not uncritical.

Peter Dennis has done extensive research in Australian, British, French, American, and, to a limited extent, Dutch archives and personal collections. He has not, however, consulted Japanese or Indian sources nor some American records he might have examined, and he has also overlooked a number of important secondary publications. His failure to use Japanese materials—or works in English based on them—is probably the biggest drawback. Although Dennis describes Japanese

activities at some length, their impact on Southeast Asia at this time was far more significant than he has managed to convey. The omission of Indian sources is also disappointing, since Indian Army units were Mountbatten's primary military resource. Finally, further research in American records might have blunted some of the author's criticisms of U.S. policies and saved him from factual errors.

Troubled Days of Peace—the words used by Mountbatten to describe this period—is thus less comprehensive and effective than it might have been. It is a welcome supplement to the existing literature, offering additional information and perceptive analysis, especially on the British political and diplomatic background. But it is not the final word on the important history of Southeast Asia in those critical months of 1945–46.

STANLEY L. FALK
Alexandria, Virginia

DOUGLAS PIKE. *Vietnam and the Soviet Union: Anatomy of an Alliance*. Boulder, Colo.: Westview. 1987. Pp. xvi, 271. \$29.85.

To read many books about Vietnam is like probing an open wound, but not this one. Douglas Pike has produced a valuable work, unemotional to the point of flatness, using Vietnamese materials extensively, sources in Russian much less, but sources on the Soviet Union and the People's Republic of China amply, much of it all drawn from Berkeley's Indochina Archive. Finding not "a single English-language full-length history of Soviet-Vietnamese relations," he decided to write one. It is brief but thorough.

Curiously, he gives us no connected narrative, long or short, of the career of his central figure, Ho Chi Minh. Not a word is said about his early life, departure from his homeland (1912), and arrival in Paris—the real beginning of Pike's story—or about what happened to him in the next seven years, during which he joined the French Socialist party. In 1919 it seems that Ho began to study Marx and Lenin, and when the party split in December 1920 he became a charter member of the French Communist party. (Marxism in Vietnam, as in China and other Asian countries, began only after the October revolution in Russia.) Only in 1930, under instructions from the Comintern, a functionary of which he was for many years, did Ho convene a conference that organized the Indochinese Communist party (ICP). In 1945 he proclaimed a Democratic Republic of Vietnam (DRV).

There ensued what Pike calls the Viet Minh war (1945–54), at the outset of which the ICP dissolved

itself to permit the Viet Minh to be set up as a united front (Communists plus others). During the war the Soviet Union supported the Viet Minh covertly and helped make the peace of 1954, intended as temporary. The Vietnam War was launched by the Communists in May 1959. Pike distinguishes three phases: 1959–64, during which Nikita Khrushchev cooled significantly on aid to Vietnam; 1964–68, when Soviet-Vietnamese relations improved slowly; 1968–75, when relations became close, reaching a triumphal note as the war ended—first in the abortive peace of 1973, then the victory of Hanoi in 1975.

Pike's book demonstrates beyond doubt that: Hanoi was never a puppet of Beijing (by now widely known); Hanoi today is not a puppet but a client state of Moscow (what probably needs to be known better is how close to Moscow Hanoi is); throughout the long years chronicled here, Ho and the North Vietnamese were zealously Communist, in fact committed to proletarian internationalism to a degree exceeding either of the two large Communist powers (unknown or ignored by much American public opinion up to the present time); the rulers barely conceal their scorn for Moscow, not because the Soviets violate Vietnamese national traditions but because they are considered ideologically weak and inconsistent (probably news to much of the American public); and the people despise and hate the Russians.

The book is not carefully edited. Errors appear in Russian names; the worst case is "Gen. Petrov Nikolai Alexandrovitch" (p. 217), reversed and misspelled. Solecisms appear, for example, "counsel general" instead of "consul general" (p. 217). Dates need checking: Richard Nixon visited Beijing not in July 1971 but in February 1972 (p. 92); the Cominform was founded in September not November 1947 (p. 33). There are questionable interpretations: in Europe the trend is toward centralization and unification, in Asia the opposite (what about autonomism in Belgium, Spain, France, Britain?); the "mystique of the French *colon*" is the reason the Foreign Legion could maintain order among 30 million Indochinese (a handful of British soldiers could do the same among 300 million Indians). But the flaws are minor. At the height of the war John Fairbank asked a scholarly gathering, "Where are the Vietnam specialists?" The answer was, as he implied, nowhere. We still have very few; Pike is among the foremost.

DONALD W. TREADGOLD
University of Washington

S. R. GOYAL. *Harsha and Buddhism*. Meerut, India: Kusumanjali Prakashan. 1986. Pp. xviii, 184. Rs. 150.

S. R. Goyal is perhaps the most prolific historian of ancient India working today. He already has twelve books to his credit (four in English and eight in Hindi) and four more forthcoming. *Harsha and Buddhism* is a most stimulating book on a very important topic. Its main thesis is that, contrary to general historical opinion, King Harsha (606–47 A.D.) has been famous in India and Asia primarily as a Buddhist ruler. This is based, he says, almost entirely on the evidence provided by the Chinese traveler-scholar monk, Hsüan Tsang (602–64 A.D.), who is known to be not only a partisan Buddhist enthusiast but also an egotist.

The book contains five parts and fourteen chapters, two appendixes, a bibliography, and an index. In part 1 the author reproduces all the relevant evidence pertaining to Harsha's religious attitude, specifically his relationship with Buddhism. He evokes inscriptions, coins, and historical and literary passages from original Sanskrit and Chinese (in translation) sources. Part 2 presents a careful analysis of the evidence, point by point, quoting other scholars' opinions, especially those of eight historians who have written books on Harsha. In a brief part 3 Goyal makes the assessment that "Harsha never adopted Buddhism as his personal religion" (p. 117). Harsha's own coins, seals, and inscriptions and Bāṇa's testimony all "prove beyond any doubt" that he was personally a Śaiva—at least until 631 A.D. That he worshipped Śiva, along with the Buddha and the sun god, Āditya, at Prayāga and Kanauj in 643 A.D., reveals Harsha as still a Śaivite when Hsüan Tsang was about to depart. There is nothing in his three plays to indicate a personal faith in Buddhism. It is no doubt true that Hsüan Tsang described Harsha as a devout Buddhist. But, taking all other evidence into consideration, it seems obvious that the learned Chinese monk simply mistook Harsha's courtesy and generosity to a foreign visitor as a sign of conversion to Buddhism. Such an attitude of generosity and catholicity of temper were, however, common characteristics of early Indian kings.

In the following section the politico-economic aspects of Harsha's relationship with Buddhism and other religions are described. Nālandā, at this time, was a huge and well-endowed university, bustling with the activities of ten thousand students and over fifteen hundred faculty—all of them provided with free room and board, clothing, and medicine (p. 127). Yet the author feels that there was a waste of rich resources on charity and warfare in Harsha's days that could have been better used in road and canal construction, agriculture, and trade development (as was the case with a number of other kings) (pp. 139–40). He believes that the subjects paid unnecessarily for

their king's penchant for merit making and personal glory. The first appendix assigns ca. 620 A.D. as the date for Bāṇa's composition of Harsha's biography, and the second describes Hsüan Tsang's travel itinerary. The book ends with a good bibliography and a serviceable index.

I noticed ten misprints (pp. 16, 28, 39, 40, 42–44, 117, 162) and was surprised to find no mention of Kanishka in the book. Although the book is printed on a good quality of paper, the binding is weak. On the whole, however, it is a well-written piece, and Goyal has made a solid contribution. His two other books on Harsha are eagerly awaited.

The work should become the standard study of Harsha's religion much as has been the case with D. Devahuti's study on Harsha's politics (*Harsha: A Political Study* [1970]).

JAGDISH P. SHARMA
University of Hawaii

R. P. SINGH. *The Central Legislature in India, 1909–1935*. Calcutta: Naya Prokash. 1984. Pp. xv, 268. Rs. 90.00.

In the field of modern Indian history, constitutional studies have become a rarity in recent times as historians have moved on to less-charted areas such as peasant studies and economic analyses. R. P. Singh has given us the revised version of a doctoral thesis in this book, which is in the older mold but shows awareness of the newer trends.

He deals with the shaping of the three important reform acts carried through by Parliament in 1909, 1919, and 1935. His focus within the sweep of constitutional change is the central legislature at each juncture. Using government records, private papers of the politicians involved, newspapers, and other materials, he tries to demonstrate why the advances were so limited.

Singh argues in his conclusion that, "What was striking about the making of the legislature at the centre was that so much effort was put in to achieve so little" (p. 248). Carefully tracing the preparatory stages for the Government of India Act of 1935, he shows that, in order to pass the bill, Secretary of State Samuel Hoare retreated before Conservative intransigents and agreed in the end to drop even direct election to the main house of the new central legislature. Singh's identification with Indian demands for self-government is built into the study and is altogether understandable several decades after that goal was achieved. It makes him, however, harsh to opponents such as Winston Churchill and sympathetic to Indian nationalists.

Although the main thread of the powers of the

central body and who could vote for whom is adhered to, Singh, perforce, also has to make reference to closely related matters such as the provincial legislatures. He must bring Mahatma Gandhi and the Indian National Congress on the stage from time to time because it was nationalist pressures and changing circumstances in India that helped move British politicians and officials to act. He is not concerned with what business the central legislature, given its extremely limited powers, conducted in these decades.

Although Singh does not take us much beyond the excellent studies by R. J. Moore, particularly *The Crisis of Indian Unity, 1917–1940* (1974), his focus on the central legislature provides a continuous story concerning this feature of constitutional advance and shows how hard it was for Indian nationalists to wring any significant power from their rulers.

LEONARD A. GORDON
Brooklyn College

M. C. RICKLEFS, editor. *Chinese Muslims in Java in the Fifteenth and Sixteenth Centuries: The Malay Annals of Sēmarang and Cērbon*. Translated by H. J. DE GRAAF and TH. G. PIGEAUD. (Monash Papers on Southeast Asia, number 12.) Melbourne: Ruskin. 1984. Pp. xiii, 221. \$6.00.

The semilegendary labyrinth of pre-Dutch Javanese historiography has thrown up another bizarre puzzle. In 1964 a Dutch-educated Sumatran engineer named Parlindungan produced a rambling, speculative, whimsical, and often pornographic book called *Tuanku Rao*. Besides much that clearly was derived from his own imagination, it included a variety of historical material allegedly obtained from C. Poortman, a former colonial official in the Batak area who rose to be resident of Jambi in the 1920s, met Parlindungan in retirement in Holland, and died in 1951. An appendix to *Tuanku Rao* purports to be Poortman's reconstruction of the Islamization of Java based on Chinese documents he had seized from the old Sam Po Kong (Cheng Ho) Temple in Semarang in 1928. It was not taken seriously by most scholars at the time, partly because it was written in the comical English-Indonesian style of the whole book (for example, "Muslim/Hanafi Chinese communities itu pun turut degenerated" [p. 656]).

The exception was Slametmuljana, who used Parlindungan's material in books in Indonesian (1968) and English (1976), calling it an abridged version of "The Chinese chronicles of Semarang and Talang." His first book outraged Indonesian Muslims and nationalists by identifying some of their traditional cultural heroes as Chinese and was banned in 1971.

Now, however, the Parlindungan material carries qualified endorsement from the three most accomplished modern historians of Java. H. J. De Graaf, who died in 1984, and Th. G. Pigeaud between them produced a dozen books reconstructing the history of Java in the period 1300–1700. None of these used Parlindungan. In their last active years, however, when both were around eighty, the two scholars noted that he appeared to confirm at least two of their independent conclusions: that Majapahit was finally conquered in 1527 (not 1478 as Javanese tradition asserts) and that the first ruler of Demak was a Chinese Muslim trader (not a son of the last king of Majapahit). They produced an English translation of Parlindungan's material and discursive comments, notes referring to the comments, and a recapitulation. For those unfamiliar with their more systematic early work (mostly in Dutch), there is much interesting erudition here, however poorly organized and documented. Far from dispelling the mystery surrounding the Parlindungan material, however, they introduce further confusion by labeling it throughout "The Malay Annals." Apart from having been preempted to designate the best-known classical Malay text, this is a strange misnomer for Parlindungan's Indonesian-English appendix, almost certainly his own translation or reworking of Poortman's Dutch.

De Graaf and Pigeaud are convinced of "the Chinese origin of the greater part of the annals" (p. 2). They speculate that in the 1740s, when Java was disturbed by a Chinese rebellion, some Chinese resident of Semarang "may have," "perhaps" compiled old Chinese documents and legends into a Sino-Malay narrative. This unsubstantiated hypothesis then develops a life of its own, and the two Dutch scholars speak of an eighteenth-century Chinese Muslim as author and of Poortman and Parlindungan as editors. Their comments are concerned to distinguish what is "acceptable" or "genuine" in the material from what is "unacceptable" or a "20th Century interpolation." By this process they demonstrate that many revelations of Parlindungan and Poortman are confirmed by other sources, some of which could not have been available to a twentieth-century forger of the material. It must therefore be taken seriously by scholars.

M. C. Ricklefs undertook to prepare the manuscript of De Graaf and Pigeaud for publication out of piety toward these two distinguished Javanists rather than a desire to authenticate Parlindungan. He distances himself more than they from the text but offers no reassessment of his own. None of the three scholars has made an attempt to trace the antecedents of Parlindungan's material. Mystery

will therefore continue until someone undertakes the systematic study it appears to warrant.

ANTHONY REID

Australian National University

NORMAN HARPER. *A Great and Powerful Friend: A Study of Australian-American Relations between 1900 and 1975*. Manchester, N.H.: University of Queensland Press. 1987. Pp. x, 416. \$32.50.

In 1908 the imposing American Great White Fleet visited Sydney and Melbourne. Most Australians exulted, expressing feelings of kinship and hopes for their enhanced security from a United States thought to be assuming a protective role in the Pacific on behalf of white and civilized societies. In 1988, celebrating the bicentennial of its European settlement, Australia is a very different place than it was eighty years earlier. It is more populous, stronger, more self-confident, far more ethnically and racially diverse and is a fully independent international actor. Its traditional reliance on British security protection was exploded in World War II and replaced by reliance on the United States. Dependence on America for defense remains to this day, but with a sense of proportion, and not without frequent dissonances in the relationship.

Norman Harper's book concludes in the mid-1970s, with the end of the Vietnam conflict. It nevertheless provides a comprehensive treatment of the gyrations over three-quarters of a century to which the Australian-American connection has been subjected—to a point where Australia's political and security importance for the United States is arguably as salient as America's is for Australia. Harper imposes no grand, explanatory scheme. There is no unmistakable thesis here. His chapter-length perspective is more a summary than an analytical reconstruction. But a consistent theme does emerge: the efforts of an unusual society, unusually situated, to have its special interests recognized and its well-being sustained by a power or powers much more formidable than it. Throughout its relationship with the United States, Australia has experienced disappointment. But it has never had cause to become so disaffected as to find it expedient to renounce close ties. It at times has elected acquiescence or followership, not mindlessly but as part of a calculus of costs and benefits. Especially since the waning years of the Vietnam War, where Harper's book ends but whose consequences it anticipates, its margin for maneuver has risen perceptibly.

Harper died shortly before the book's publication, leaving a legacy as one of the pioneers of and foremost contributors to the history of Australian-

American relations. This book was to be his *pièce de résistance*. It is more massive and detailed than the few other available Australian-American histories. Its research is exhaustive, reflecting careful reading of official and private archival sources as well as of secondary literature and the eye of a seasoned observer.

This, however, is not a study of Australian-American relations in their entirety. There is a fine chapter on the trade diversion dispute of the 1930s but otherwise little on economic interfaces, including more contemporary trade and investment developments that have colored other features of the relationship. America's cultural effects on Australia are only incidentally broached, and the policy processes and politics behind Australia's outlook on and policies toward America are likewise underplayed.

But it would be churlish to fault Harper for what he did not set out to do, the book he did not intend to write. Taking the book on its design, which is diplomatic history assembled by coherent chronological segments, we have a rich and excellent study. All the main historical ingredients, and a number of lesser ones, are here: Australia's resolute pursuit of its perceived interests within the councils that shaped the two postwar settlements; its historical wariness of danger from Asian powers, from Japan to China to Indonesia; the trade-offs weighed between deploying troops in support of powerful friends far afield and looking more closely to its more immediate security environment. Harper has been less interested in indicting or celebrating Australia or the United States than in examining the penumbra of change across axes of continuity.

HENRY S. ALBINSKI

Pennsylvania State University

UNITED STATES

MICHAEL KAMMEN. *Selvages and Biases: The Fabric of History in American Culture*. Ithaca: Cornell University Press. 1987. Pp. xv, 336. \$24.95.

Are several "Michael Kammen" clones scurrying about the library and sitting at word processors up there in Ithaca? To this ordinary mortal, it sometimes seems so. During the same interval when he was writing *A Machine That Would Go of Itself* (1986), his magisterial cultural history of the American Constitution, Kammen also managed to present the Curti Lectures at the University of Wisconsin (published in 1986 as *Spheres of Liberty*) and to produce the twelve essays and invited lectures collected in this handsomely illustrated and wittily titled volume.

The three most important pieces, which alone would justify this publication, are the sixty-four-page "Historical Knowledge and Understanding," originally delivered at a University of Chicago symposium on social science paradigms; "Vanitas and the Historian's Vocation," first published in *Reviews in American History*; and an essay on the state of American cultural history that began life as an invited paper at a conference of the Organization of American Historians. But the book also includes several essays (one in collaboration with Carol Kammen) on the pitfalls and rewards of local history, an interesting study of changing perceptions of the life cycle in American thought, several short pieces that first appeared in the *New York Times*, and perceptive interpretive essays on the historians Moses Coit Tyler and Johan Huizinga. (When Tyler was appointed professor of American history at Cornell in 1881, Kammen tells us, one trustee complained that this was carrying specialization entirely too far.)

In their very diversity these essays illustrate the range of Kammen's scholarly interests, his integrative skills, and the breadth of his research—research that encompasses such arcane sources as William Lambarde's *A Perambulation of Kent* (1576); the papers of the 1909 Hudson-Fulton Celebration in New York State; "an obscure little essay published in England by the Middlesex Local History Council in 1963"; the January 1940 *Rotarian*; and the autobiography of Hoagy Carmichael. Sometimes, it must be said, the display of erudition is carried a bit far. In the first seven paragraphs of the preface, for example, in justifying his decision to collect these essays, Kammen manages to quote from Huizinga, H. R. Trevor-Roper, Samuel Eliot Morison, Oliver Wendell Holmes, Sr., Edmund Burke, and Johan Georg Hamann. And the essay on Tyler bogs down a bit in a series of extended comparisons with other mid-Victorian historians.

But, although Kammen's muse occasionally nods and despite the unevenness inevitable in a collection of essays on different topics produced for different occasions, this book well repays reading. "Historical Knowledge and Understanding" expertly describes how historians approach their craft and offers an incisive critique of the heavily theoretical and ahistorical quality of some social science research. ("Social science without a historical dimension resembles those larger-than-life yet ludicrous caricatures on parade floats: superficially grand but lacking in substance and all too easily shredded" [p. 34].) This essay would provide an excellent introduction to historical thinking for students in historiography courses.

"Vanitas and the Historian's Vocation," more personal in tone, explores with sensitivity the

doubt and uncertainty (and occasional fatuousness) that have characterized even the most celebrated historians, but it ends with an eloquent statement of the importance of history and the satisfactions of historical scholarship. Kammen's indefatigable zest for research in the papers of American historians adds a special depth and richness to this essay, with telling quotes from Charles M. Andrews, Richard Hofstadter, J. Franklin Jameson, Frederick Jackson Turner, Albert Bushnell Hart, and others.

Kammen clearly feels affection and concern for the scholarly discipline to which he has contributed so prolifically; he cares about the historical profession and its practitioners. Not the least of the virtues of this book is the way it illuminates this appealing side of its author's personality.

PAUL BOYER

University of Wisconsin

BARBARA L. TISCHLER. *An American Music: The Search for an American Musical Identity*. New York: Oxford University Press. 1986. Pp. viii, 225. \$24.95.

In recent years we have had several scholarly books about the attempts of American "classical" composers to find a national cultural identity and to compose recognizably "American" music: MacDonald Smith Moore's *Yankee Blues: Musical Culture and American Identity* (1985) and Alan Howard Levy's *Musical Nationalism: American Composers' Search for Identity* (1983), both of which focus on the 1920s; and Barbara A. Zuck's *A History of Musical Americanism* (1980), which concentrates on the depression and World War II. Now we have, on the same subject, Barbara L. Tischler's book, which is broader in scope than the other works—so broad, indeed, as to seem almost like a collection of essays on disparate topics. The author frequently strays from her announced subject, and her discussion is sometimes more anecdotal than analytical.

The great musical task for America in the nineteenth century, she argues, was just to raise the level of public taste to the point where it could appreciate European art music. There was little time or energy left for promoting a recognizably "American" music, and American composers tended to imitate the European (especially German) romantic style of the time. The American peace jubilees, expositions, and world's fairs that were held in the late nineteenth and early twentieth centuries, although they were supposed to be celebrating American achievement, actually relied on European music or music by Americans that was imitative of European styles. And when, at the

beginning of the twentieth century, some composers sought to sound "American" even they did so by rather incongruously quoting Indian, black, and Anglo-American folk tunes within a musical context of European romanticism. During World War I, when proponents of "one-hundred-percent Americanism" sought to remove German music from America's concert halls and opera houses, their efforts proved short-lived and ultimately ineffectual. German romantic music was simply too central to the American audience's idea of what constituted "good music."

When Tischler reaches the 1920s and Aaron Copland's generation of composers, her treatment becomes more meaty, although the story is already familiar enough from the previously published books mentioned above. Her principal thesis is that Copland's generation marked the point when American composition "came of age" because these composers rejected European romanticism and embraced the international modern music movement (usually through study in Europe) not because they rejected European influences in general and turned to America for inspiration. And even later, during the Great Depression and World War II, when members of this generation adopted social concerns, sought to simplify their works in order to reach a broader audience, and attempted to sound more "American," they did so in compositions that embodied the techniques of musical modernism. There is a chapter on the relations of the federal government with music, particularly the New Deal's musical activities, and the author concludes by discussing two new sources of income that have been progressively opened up to American composers in the twentieth century—composing film scores and teaching in colleges and universities.

Unfortunately, this book is full of small errors—typographical errors, incorrect information about works cited, and misstatements of fact.

FRANK ROSSITER
University of Texas,
Dallas

CHRISTINE BOSE *et al.*, editors. *Hidden Aspects of Women's Work*. Assisted by the WOMEN AND WORK RESEARCH GROUP. New York: Praeger. 1987. Pp. x, 380. \$35.95.

This volume represents the collective efforts of the Women and Work Research Group—a gathering of sociologists, a few historians, and other social scientists who began meeting in 1979 to study issues related to gender and employment. Five of the book's thirteen essays have been published before, although they are revised here. Although

some of the essays offer more to sociologists and students of contemporary labor conditions, other chapters are valuable for historians of women and work as well.

The introduction suggests that the authors would make visible previously hidden aspects of women's work. These include job routinization, employment barriers, sexual harassment, and the intersections between work and home life. This introductory essay links together the articles in the book's three parts by recounting themes, sub-themes, and "issues that crosscut the themes" (p. 8). This linkage is difficult because of the widely diverse, though often rich, essay subjects.

The book's first part focuses on systematic and institutional dimensions of women's work. Evelyn Nakano Glenn's essay on the labor of minority women takes a Marxist-feminist approach to the study of race and gender stratification, comparing the historical situation of Mexican Americans, the Chinese in California, and southern blacks. Glenn and Roslyn Feldberg's revised chapter on the effect of office automation examines clerical work from 1960 to 1980 and suggests in part that women more than men have been negatively affected by office automation. An intriguing essay by Christine Bose analyzes the census count of employed women in 1900 and 1980 and argues that changing definitions of employment rather than accelerating work participation rates account for what is widely perceived as the rapid growth in the female labor force. Although this conclusion is controversial, the emphasis on the ideological basis of census definitions is a useful caution for those who work with census figures. The section also includes interesting articles by Natalie Sokoloff on the labor struggles facing women in the 1980s and 1990s and by Amy Gilman on the ideology of women's work in mid-nineteenth-century fiction.

The second section, titled "Links between the Public and Private Spheres," opens with essays by Carol Brown on patriarchy in the family and in the public arena and by Maxine Marx Ferree on the "Superwoman" ideology and its relationship to feminism and public policy. Carol Turbin finds that for Troy, New York, from 1860 to 1890 the analysis of family life enhances the understanding not only of women's work but also of labor activism. The final essay in this section is a look by Susan Lehrer at minimum-wage legislation for women from 1910 to 1925.

The last third of the book, "Consciousness and Resistance," explores popular ideas about female workers and traces attempts to resist or change these images. This section opens with an investigation by Peggy Crull of sexual harassment. A revision of Glenn's excellent article on Japanese-

American women and domestic service from 1905 to 1940 discusses Issei women and domestic work in prewar years. Amy Kesselman's study of female shipyard workers after World War II creatively uses oral history and government records to explore women's resistance to demobilization. In the last article Feldberg criticizes the standard question of why women have resisted trade union involvement. Instead, she argues, scholars should ask how unions could change to enable women to become more active.

This collection offers a pastiche of essays that vary greatly in methodology, scope, and significance. The topical rather than chronological arrangement is not as useful for historians as it might be for other social scientists, particularly because there are no separate introductions to the three sections of the book. Additionally, many of the "hidden" aspects of work have already been uncovered by recent scholarship in the field. This scholarship includes the earlier versions of these essays, which comprise some of the strongest contributions to the collection. Historians of women and work, however, will find both new and old material stimulating, not least because of the interdisciplinary focus of the Women and Work Research Group.

LYNN WEINER
Roosevelt University

DANIEL K. RICHTER and JAMES H. MERRELL, editors. *Beyond the Covenant Chain: The Iroquois and Their Neighbors in Indian North America, 1600–1800*. Foreword by WILCOMB E. WASHBURN. (Iroquois Books.) Syracuse, N.Y.: Syracuse University Press. 1987. Pp. xiv, 211. \$27.50.

Following publication of *Extending the Rafters: Interdisciplinary Approaches to Iroquoian Studies* (edited by Michael K. Foster *et al.* [1984]) and *The History and Culture of Iroquois Diplomacy* (edited by Francis Jennings *et al.* [1985]), this is the third collection of essays on the Iroquois to appear in almost as many years. Growing out of the "Imperial Iroquois" conference held in Williamsburg, Virginia, in 1984, the essays in this volume look beyond the covenant chain of Iroquois-European alliances, focus on Iroquois relations with their Indian neighbors, and reassess the Iroquois's dominant role in native North America.

Daniel K. Richter's opening chapter traces the gradual emergence of confederacy unity and underscores the distinction between the Great League of Peace, the cultural and ritual institution organized by Deganawidah and Hiawatha, and the Iroquois Confederacy, which developed as a political and diplomatic entity after European contact.

Richter sets the tone for the volume: the Iroquois were localistic villagers confronting common crises, not stoic statesmen confident of their military and diplomatic dominance. Mary A. Druke analyzes the general structure of Iroquois intertribal diplomacy, explaining that reciprocal relations between leaders and followers were as important in intertribal councils as in Iroquois villages. Richard L. Haan argues the need for a further revision of interpretations of the covenant chain to better reflect the Iroquois perspective.

The essays in part 2 consider Iroquois contacts with their immediate neighbors. Neal Salisbury's description of reciprocal relations with Algonquians provides an antidote to the notion that the Iroquois terrorized Indian New England. Francis Jennings finds evidence among the Susquehannocks and Delawares in Pennsylvania to support his view expressed in *The Ambiguous Iroquois Empire* (1984) that Iroquois forest hegemony was a myth fabricated by the English, and Michael McConnell shows that the Six Nations enjoyed only limited influence over the migratory tribes of the Ohio Valley even before the Treaty of Fort Stanwix in 1768.

Looking south, James H. Merrell shows that recurrent warfare with the Iroquois occupied a central place in Catawba culture. Far from offering marauding Iroquois easy prey, Catawbans established their own reputation for ferocity and treachery. Theda Perdue explains how eighteenth-century Cherokees and Iroquois conducted their relations to suit their own purposes while simultaneously taking account of British power and pretensions. Douglas W. Boyce shows that ethnic and linguistic kinship was no guarantee of cooperation between Tuscaroras and Iroquois before Tuscarora refugees migrated north at the beginning of the eighteenth century to find a haven and a gradually improving status as adopted members of the confederacy.

Anyone who has steered a multiauthored volume to publication deserves credit for the achievement rather than criticism for what it lacks, and the editors acknowledge that this is a sampling of Iroquois relations with their numerous neighbors. Still, it is unfortunate the sample could not include a study of Iroquois encounters with the Hurons, an area crucial to establishing the Iroquois military reputation, an exploration of the relationship with their Caughnawaga relatives who became intermediaries for the Seven Nations of Canada, or an examination of relations with the Ottawas, Algonquians, and Montagnais who figured so prominently in seventeenth-century fur trade networks. The essays focus exclusively on the area within the present United States. Without the balance of a Canadian dimension, the volume presents a better

view from Williamsburg than from Iroquoia. It is also thin on the critical era of the American revolution.

The reports from the north are not in, and access to the microfilm publication *Iroquois Indians: A Documentary History* (1984) may produce new evidence. But these fine studies of Indian-Indian relations provide a more accurate picture of Iroquois power and presence in native North America and demonstrate that the field of Iroquois history is far from overworked.

COLIN G. CALLOWAY
University of Wyoming

BRUCE H. MANN. *Neighbors and Strangers: Law and Community in Early Connecticut*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press, in association with the American Society for Legal History. 1987. Pp. xi, 202. \$27.50.

In the second edition of his monumental *A History of American Law*, Lawrence Friedman judged legal history to be "on the way to maturity." No better proof of that judgment can be found than Bruce H. Mann's subtle, lucid, thoughtful, and convincing study of debt law and commercialization in colonial Connecticut. At one level the story is easily told. As the colony changed from a congeries of subsistence farming villages to a network of commercial farms clustered around growing towns, "debt litigation became formalistic and unforgiving, pleading grew more technical and less expressive of the facts of individual disputes, and the civil jury faded from predominance to comparative insignificance . . . the changes marked a transformation from a legal system that allowed litigants to address their grievances in ways that were essentially communal to one that elevated predictability and uniformity of legal relations over responsiveness to individual communities" (pp. 9-10).

At the heart of this tale of the downside of modernization is the fate of "book debt." When much of the economic exchange within a community was based on barter of services for credit, merchant storekeepers kept records of indebtedness in ledger books, which could be brought to court as evidence, rebuttable to be sure, that a debt was owed. This alternative to the cumbersome common-law action of debt presented the issue to juries in an informal fashion. The jury then determined whether the defendant's answer, for instance, that the debt had been repaid but the repayment not entered in the book, was creditable. Neither side needed trained counsel to draw up formal writs or argue the issue before a jury of their neighbors. Whither book debt, so the infor-

mal character of Connecticut civil dispute resolution.

I have oversimplified a highly nuanced, thoroughly researched argument. Mann has tabulated the results of thousands of cases from New Haven and New London superior courts, other county courts, and the colonial general court, without losing sight of the colorful detail of individual cases such as Stephen Pierson and Stephen Parkins's bitter dispute over a strayed gray horse—the very stuff of which lawsuits are made. Indeed, Mann may be categorized as a leading exponent of a "new, new" legal history. If the old legal history was organicist and internalist, and the "new" legal history was contextual and externalist, Mann, with Michael Grossberg, Hendrik Hartog, N. E. H. Hull, David Konig, William Nelson, and Marylynn Salmon, among others, here blends a sure grasp of the internal workings of private law with great sensitivity to the changing social setting of civil disputes.

In the end Mann's conclusions do not surprise us. He has found powerful confirmation of a transformation from *Gemeinschaft* to *Gesellschaft* that overtook Americans in the eighteenth century. The dispute among historians of early America, James Henretta and Christine Heyrman, on the one side, and James Lemon and Richard Brown, on the other, to name but four of the protagonists, concerns the timing of the shift from premodern markets and communally sanctioned arbitration to modern commercial transactions and formalistic legal contests. Was it well on the way by midcentury, or did it come only after the revolution? Mann, with Richard Bushman, appears to put Connecticut in the camp of the early modernization school. Like the ablest jurist expanding his jurisdiction, Mann also claims a special place in this study for the legal historian. "One consequence of the divergence of law and community was that the formal legal system became the standard for all forms of disputing" (p. 168). The implication of Mann's conclusion is that, henceforth, all entrants in the debate over modernization must include legal history in their calculations. Were that dictum established by usage, legal history would indeed be fully mature. In this book Mann has moved us much closer to that goal.

PETER CHARLES HOFFER
University of Georgia

ROBERT STANSBURY LAMBERT. *South Carolina Loyalists in the American Revolution*. Columbia: University of South Carolina Press. 1987. Pp. x, 352. \$29.95.

This book is about the 22 percent of South Carolina's white population of seventy thousand who

were loyalists. Robert Stansbury Lambert defines loyalists as those who acted either to oppose patriot actions or to support the British. The largest group lived in the backcountry, and many were recently arrived immigrants, not fully integrated into colonial society. The coastal loyalists were a minority and included royal officeholders, Scots businessmen, and professionals.

Loyalist strength in the backcountry explains the military events between 1775 and 1782, which are the focal point of the book. In 1775 there was widespread backcountry opposition to patriot actions and restiveness among the Indians. By 1777, however, the patriots had crushed the backcountry resistance, signed a treaty with the Cherokee, and established a new state government in South Carolina. Yet, the British overestimated the strength and zeal of backcountry loyalists and in 1779 and 1780 invaded the region as part of an overall strategy to recover the South.

By 1781 it was apparent that the British strategy to hold the interior had failed, and the loyalists who had declared themselves were forced to flee to an overcrowded, expensive, and at times disease-ridden Charleston. Eventually, these exiles would join earlier ones who had fled to Britain and other parts of the empire. Many who made the unfortunate choice of moving to Florida had to uproot themselves again when the British transferred the territory to Spain. Loyalists who remained in South Carolina were treated less harshly than elsewhere because of the intervention of friends and relatives and the number of so-called protectionists, who had cooperated with the British between 1780 and 1782 when they controlled Charleston.

Lambert is at his best when describing the military campaigns and their effects. He relies heavily on primary material to detail the military events, the loyalist-patriot civil war in the backcountry, and the willingness of many pragmatically to change sides as the winds of war changed, which raises interesting questions about the supposed sanctity of oaths. Even these sections would have been improved if there were less detail, tighter organization, and more maps accompanying the discussion of military campaigns.

Most significant perhaps is what is missing. There were 80,000 to 100,000 blacks among a white population of 70,000, and of the loyalist exiles 7,200 were blacks and 6,700 whites, yet only about ten pages of the book are devoted to blacks. Moreover, blacks and women are considered by themselves rather than being integrated into the narrative. Regarding loyalist women, Lambert describes ways in which they were discriminated against, their capacity to adjust to new and demanding circumstances, and the omnipresence of

death in their lives. But women also get short shrift, meriting only about five pages. Finally, considering the records available, some interesting statistical analysis would have been possible.

There are other senses in which the scope of the book is too narrow. Loyalism in South Carolina is not placed in the broader context of loyalist historiography. Intriguing comparisons that could have been made with other loyalist groups are overlooked. And the secondary literature in the bibliography is somewhat dated, with few titles from the 1980s, and key books, such as James W. Walker's work on black loyalists, are omitted.

Despite its shortcomings, Lambert's book tells us a great deal about South Carolina loyalists and the military struggle to control the colony. It affirms once again the value of monographs in understanding the revolution.

JANICE POTTER
University of Saskatchewan

CYNTHIA DUBIN EDELBURG. *Jonathan Odell: Loyalist Poet of the American Revolution*. Durham: Duke University Press. 1987. Pp. xv, 205. \$24.95.

In her preface to this biography of Jonathan Odell, Cynthia Dubin Edelberg notes that her subject was "one of the most influential and gifted of the Loyalist writers" (p. xi). Few readers familiar with Odell's witty poems would quarrel with that assessment. Yet few historians will also be fully satisfied with this biography of Odell, a physician and Anglican clergyman from New Jersey whose chief claims to fame are his close association with the British and loyalist elite in occupied New York City during the revolution and the poems and essays he produced both before and during that experience.

Edelberg, a professor of English, is more interested in her subject as a literary figure than as a historical actor. That in itself would not pose a problem. What will cause historians who specialize in the revolution or in the loyalists to wince, though, is Edelberg's failure to place Odell in an adequate context or even sometimes to get her facts straight as she discusses Odell's career and the events that produced it.

Odell himself is a promising subject. No one, after all, could entirely dislike the man who produced the classic "epitaph" that compared Benjamin Franklin to his stove: "Here lies the renowned inventor . . . /Whose flame to the skies ought to burn/But inverted descends to the center." No rebel was free from Odell's pointed pen. For example he had this to say about Thomas Paine: "Swarms of deceivers, practis'd in the trade,/Were sent abroad to gull, cajole, persuade . . . /Pervert all

records sacred and profane:/And chief among them stands the villain Paine" (p. 101). Unfortunately, when Odell turned to praise of the British rather than criticism of his enemies, his poems lost their bite; those works are dull and uninteresting by comparison.

Edelberg, making use of hitherto underused manuscript collections in Canada, expands the canon of known Odell writings, attributing to him several important series of published essays as well as previously unpublished verse. In that she has done an important service. Yet, ultimately, this book is disappointing because it lacks compelling historical analysis. It is also replete with errors, some minor, some more crucial. Edelberg, for example, believes too much loyalist rhetoric about the prevalence of loyalism in New York and South Carolina, and, furthermore, she tells us that the American population was "largely illiterate" at the time of the revolution (p. 24) and that the loyalist claims commission, which had no power to appoint anyone to anything, somehow named Odell secretary of the province of New Brunswick (pp. xiv, 153). We are, moreover, introduced to such persons as Issac Sears, Lord Germain, Lord Shelbourne, and William Tyron [all *sic*]. In short, for those who want to know more about Odell's writings, this book will prove useful; those looking for an insightful discussion of loyalism and its adherents should look elsewhere.

MARY BETH NORTON
Cornell University

CHARLES R. KESLER, editor. *Saving the Revolution: The Federalist Papers and the American Founding*. New York: Free Press of Macmillan. 1987. Pp. ix, 334. \$29.95.

This collection of essays on the meaning and interpretation of *The Federalist Papers* will frustrate most historians. Many of the papers are exercises in *explication de texte*. Most accept the totemic status that *The Federalist* has recently attained and exhibit the aridity of so much internalistic textual analysis that follows the lead of Leo Strauss. Like Biblical exegetes—and clearly contrary to their desires—the authors pursue a course that tends to diminish rather than to quicken these great texts of the American science of politics.

One of the method's chief deadening effects is that, in this case, it wrests Publius from context and leaves his work as disembodied statements without apparent referents in society and culture. In addition many of the authors disregard the pathbreaking historical literature of the last twenty-five years. William Kristol, for instance, in an essay on the separation of powers, does not cite

W. B. Gwyn, M. J. C. Vile, or Gordon S. Wood on that subject. A final problem is the suspicion some of the authors arouse that it is the cart of argument that leads the horse of evidence rather than the other way around; here, ideological purpose is only thinly veiled in insistent protests that we must avoid ideology. Nevertheless, historians may find some value in essays on Publius's views of republicanism, political life, foreign policy, the separation of powers, justice, the rule of law, and the presidency and in the respectful evaluations, especially those of Charles R. Kesler, of the pathbreaking, influential scholarship of Douglass Adair and Martin Diamond.

The most notable of the fourteen essays are those by Murray Dry, Jeremy Rabkin, James Stoner, Ralph A. Rossum, and Jack N. Rakove. Dry, in keeping with the recent resurrection of the Antifederalists, presents *The Federalist Papers* as a counterpoint to the principal Antifederalist writings—the "Letters of Brutus" and "Letters of the Federal Farmer." In thus seeing the debate between the authors of these three great sets of essays whole, Dry shows how the Antifederalists' demand for a Bill of Rights was part of their debate with Publius over federalism. In a related paper Rossum insists, contrary to conventional approaches, that Publius conceived the Constitution itself as a bill of individual rights as well as a plan for republican government. Yet here is a case in which an otherwise strong argument is marred by gratuitous presentist ideology. Rossum's purpose seems to be to devalue the weight of the Bill of Rights vis-à-vis the original Constitution from which it has come to be considered a separate document. In Rossum's view this unfortunate distinction has allowed the Bill of Rights to be used to justify judicial declarations of new rights not envisaged by the eighteenth-century Constitution.

In two other essays that will interest historians, Rabkin reviews Publius's notions of public administration and, in a critique of Weberian theory widely informed by historical reference, illustrates how *The Federalist's* view of bureaucracy and public administration differs from modern theories of the same subject. In the second, Stoner offers a distinctive argument that Publius was indebted to the tradition and practice of the common law for many of his views about the role of the courts and the function of judging.

Rakove's concise essay is a deft review of the early acceptance of Publius as an authority on the Constitution and constitutionalism. The principal weight of his argument, supported in an accompanying paper by Dennis J. Mahoney, is to show that *The Federalist* became the subject of intensive scholarly study and debate only in this century, principally because of the research of Charles A.

Beard and Adair. In fact, if witness is needed that these eighteenth-century debates remain vital still, this collection of essays provides it.

JAMES M. BANNER, JR.
Washington, D.C.

STEVEN WATTS. *The Republic Reborn: War and the Making of Liberal America, 1790–1820*. (New Studies in American Intellectual and Cultural History.) Baltimore: Johns Hopkins University Press. 1987. Pp. xxiii, 378. \$29.50.

The War of 1812 has always been a curiosity in American history—an inconclusive, mostly inglorious conflict that contemporaries insisted was of the utmost importance to the new nation but one that even they had difficulty explaining. The historiography of the war has been small in quantity and disproportionately concerned with establishing the war's causes. Steven Watts's ambitious book aims both to explain why contemporary Americans said what they did about the war and to recast it as a central event in the formation of the nineteenth-century republic. It deserves the attention of serious students of the early national period and of anyone interested in the transition from republican to liberal political culture in post-revolutionary America.

Watts argues that between 1790 and the first decade of the nineteenth century thoughtful Americans worried about the evident contradictions between republican ideology and their individualist, or liberal, economic behavior—a contention he illustrates with more than twenty biographical sketches of political, intellectual, literary, and religious figures. Most of these men favored going to war as a means of establishing the “national character” of the United States. By probing their personal experiences as well as their public pronouncements, Watts is able to argue that their views emerged from personal attempts to resolve inner doubts over the disparity between the austere prescriptions of republicanism and the reality of their personal lives. The war promised them—and, by extension, the nation at large—a release from guilt, an opportunity to demonstrate the kind of courageous self-sacrifice that the Founding Fathers had shown, and a means of validating liberal political and economic behavior. The successful conclusion of the war brought a great emotional purgation and established a new political economy, liberal republicanism, in a “hegemonic” position within American culture.

This robust formulation is not without its problems. Most seriously, the thesis of the book, that the War of 1812 crystallized an emergent American culture and defined the course of its develop-

ment in the nineteenth century, remains more asserted than proved. Although Watts faults previous historians for leaving “in the shadows” the “vital connections between economic change, transforming social values, developing political ideology, and the actual circumstances of the war” (p. 277), he himself devotes only one section of his last chapter—about seven pages—to analyzing the actual circumstances of the war. The rest of the book concerns what Americans thought about the prospect of military conflict and examines the psychological “compulsion to war” that underlay “the veneer of statecraft and rationalization” (p. 206). Thus, despite its broad claims to analyze “the complicated function of war in American society” (p. xxiii), the book mainly contributes a new perspective to the old debate on the causes of the war.

Beyond this, some readers may find Watts's insistence on psychological explanations for the attitudes and utterances of his witnesses to be reductionist in character, since it can make all other meanings (including the denotative meanings of the words in the texts examined) subordinate in importance. Others may object that his tendency to attribute great transforming power to “liberalization” is not much different from other scholars' attempts to make such claims for modernization. Still others may conclude that references to Antonio Gramsci, Raymond Williams, Karl Marx, and the Frankfurt school tend more to festoon than to inform the book's argument and conceptual structure. Such judgments will depend on each reader's tastes and ideological commitments. Whatever the reader's preferences, however, he or she will do well to read this book for its provocative insights into the character of American culture in the early national period.

FRED ANDERSON
University of Colorado

THEODORE J. CRACKEL. *Mr. Jefferson's Army: Political and Social Reform of the Military Establishment, 1801–1809*. (American Social Experience Series, number 6.) New York: New York University Press. 1987. Pp. xiii, 250. \$35.00.

Thomas Jefferson has never been considered a friend of regular soldiers. Even sympathetic scholars pronounce his army policies naive, a product of his whig prejudices. Theodore J. Crackel assaults but cannot erase this view. He portrays a Jefferson deeply concerned about military affairs because of his ideologically fueled republican nationalism and partisan fears. Crackel argues that President Jefferson, fearing a potentially disloyal army led by fiercely Federalist officers, embarked on a far-reaching reform after 1801. Jefferson

could not purge the officer corps, but, as with the federal courts and bureaucracy, he inserted staunch Republicans where he could.

As secretary of war, Henry Dearborn was Jefferson's chief military deputy. Dearborn translated Jefferson's political and military ideas into administrative reality and collaborated in drawing up the Military Peace Establishment Act of 1802. Crackel details Dearborn's recruiting efforts and the founding of a military academy and concludes that by 1808 Dearborn and Jefferson had republicanized the army. Despite Dearborn's mania for detail and genuine capacity, however, the War of 1812 would reveal that republicanization had dubious consequences for military preparedness. The author also defends James Wilkinson's loyalty as commander of the southwestern army in the context of the Louisiana Purchase and Aaron Burr's shadowy schemes.

Crackel's clear thesis and arguments rest on impressive research in manuscript sources. At the same time the author seems overly aggressive about his revisionism. Crackel's interpretations also occasionally appear labored. Army disloyalty, for example, is more asserted than proven. Federalists did vastly outnumber Republicans in Alexander Hamilton's provisional army, but Crackel's table on page 50 also suggests that 82 percent of those officers had no declared politics at all. And Robert Gough has shown that Federalist partisanship rarely ensured officer appointments in 1798. Finally, the ambitions and motives of those orbiting Burr's conspiracy in 1806 and 1807 remain obscure at best. Crackel's dogged defense of Wilkinson thus often reads like a legal brief—more insistent than persuasive.

Crackel effectively reconsiders some corners of Jefferson's army policy. The author thus marches with the "new" military historians who probe the cultural, social, and intellectual aspects of military affairs. He presents the best available analysis of the military legislation of 1802 and convincingly explains Jefferson's support of a military academy. Crackel admits that Jefferson's program had limited success, but the president built for the future. And Crackel does not weigh the effect of Jefferson's efforts on anti-army sentiment in Congress among the various groups that constituted the Republican party, apart from the unpleasable John Randolph.

Crackel's view of Jefferson's partisanship as near-paranoia seems sensible because Jefferson did tend to be somewhat irrational about the high Federalists. The Federalist menace was partly trumped up in the partisan atmosphere of the election of 1800. Crackel might have done more to explore the reality beneath Republican imaginations here. And neither Jefferson nor Dearborn

realized that inserting partisan counterweights would factionalize and weaken the army.

Crackel's focused monograph perforce produces restricted conclusions. But this is a useful analysis of Jeffersonian army policy, despite the author's tendency to overinterpret his evidence and act as James Wilkinson's defense counsel. Scholars of Jefferson's presidencies and military policy will want to consult this volume.

REGINALD C. STUART

Mount Saint Vincent University

ROBERT F. DALZELL, JR. *Enterprising Elite: The Boston Associates and the World They Made*. (Harvard Studies in Business History, number 40.) Cambridge: Harvard University Press. 1987. Pp. xviii, 298. \$27.50.

In 1814 a small group of wealthy Boston merchants set up a cotton textile mill at Waltham, Massachusetts. The first of its kind in America, the mill combined the processes of spinning and weaving to turn out a rough cloth that had a market in the South and West. To provide a labor force, the mill's proprietors (dubbed "the Boston Associates" by a later historian), recruited young women from rural areas, housed them in dormitories, provided essential services, and thus created the mill town that became typical of New England. Eventually, the associates numbered some eighty men, who, in addition to Waltham, founded several other mill towns, including Lowell, Lawrence, and Holyoke, Massachusetts.

Usually regarded as the forerunner of the Industrial Revolution in America, the mill at Waltham was the nucleus of the world the associates made—a world of safe investment, innovative financial institutions, selective philanthropy, and conservative politics, which together were to underwrite and preserve family and class security. Although conventional hindsight sees Francis Cabot Lowell, Amos Lawrence, Abbott Lawrence, Nathan Appleton, and their cohorts as aggressive innovators, boldly striking out for profits in a new economic sphere when trade during the War of 1812 ceased to employ their capital, Robert F. Dalzell, Jr., sees them as men intent on "the preservation of fortunes already made, positions already won" (p. 67). True, "as businessmen they were innovators operating on a scale unparalleled in America at the time," but "ultimately they hoped to check the thrust of change—to alter the world they knew only enough to make it secure for people like themselves" (p. xii).

Dalzell contends that there was money still to be made in wartime trade, risky though it was, but the associates, growing older, sought a safe investment

that would demand less personal attention. They found it in textile manufacture, copied from the British but so adapted to New England that the society they knew and their place in it, the associates planned, would be left unchanged in any significant way. Subsequently, they moved into transportation, banking, and insurance to support their textile empire and, ultimately, their social position. Their philanthropy met community needs but also buttressed the associates' social and economic position. The Massachusetts Hospital Life Insurance Company, for instance, established to finance Massachusetts General Hospital, became an early vehicle of intergenerational trust management and a source of funds for the associates' other economic ventures. Support of the Whig party and service in public office closed the circle of the associates' world.

The passing of the associates and the decline of the Waltham-Lowell system after 1845 left much of their world in place. Their innovations in banking and in the use of the corporation and the investment trust were the foundation of Boston as a financial center. Their contributions to education, museums, and libraries established Boston as "the Athens of America." Their descendants, the "proper Bostonians" or "Boston brahmins," remained a discrete group in the city's population.

Parts of the saga of the Boston Associates have been well told before, but with sharp insight Dalzell has put the story together into a logical and convincing whole. Carefully researched, bountifully detailed, and well-written, this is a very good book.

IRENE D. NEU
Indiana University

ANTHONY F. C. WALLACE. *St. Clair: A Nineteenth-Century Coal Town's Experience with a Disaster-Prone Industry*. New York: Alfred A. Knopf. 1987. Pp. xvii, 519. \$30.00.

Anthony F. C. Wallace's new book does for a Pennsylvania anthracite mining village in the Pottsville Basin what his earlier *Rockdale* (1978) did for a small textile community on the Brandywine in southeastern Pennsylvania. As in his earlier work, Wallace links a detailed analysis of local events with major intellectual, social, economic, and technological currents sweeping through nineteenth-century America. Wallace skillfully weaves the geology of the area, the economics and politics of anthracite, the technology of mining, census data, corporate records, folklore, family history, biographical sketches of key figures, court records, and much more into a complex tapestry that demonstrates the impact of the mining indus-

try on people's lives. Unlike many studies, its focus is not limited to business leaders who developed the community's economy or to workers whose lives were transformed by industrialization. The book introduces and explains entrepreneurs, coal operators, union organizers, laboring families, and those not directly involved in mining, as well as the interrelationships between these groups. Among the principal persons in the study are the Carey and Wetherill families and their allies (Philadelphia businessmen who initially opened the area to mining), Franklin Gowen (president of the Reading Railroad, which later bought up and monopolized the region), John Siney (labor leader and organizer of the Workingmen's Benevolent Association), the Molly Maguires, and a host of ordinary working-class men and women.

Wallace's central thesis is that anthracite mining in and around St. Clair was impractical from the start. Moreover, expert opinion warned that such was the case even as the promoters were opening their first mines. Henry Darwin Rogers, a professor of mineralogy and geology at the University of Pennsylvania, with his brother William, conducted the first geological survey of Pennsylvania. According to Rogers's official report in 1838, careful examination of the geological structure of the Pottsville Basin revealed that numerous faults broke up the seams of coal, making them unpredictable and both unsafe and extremely costly to mine. Several subsequent studies by disinterested scholars confirmed this finding. Such warnings fell on deaf ears. The original owners and developers instead put their trust in three "illusions" formulated by promoters and speculators: that pessimistic evaluations were only "theories" of misguided, ivory-tower professors who lacked the common sense of experienced mine operators, that low profits in the industry were due to low tariffs on British iron, which discouraged American ironmasters from expanding operations and using more coal, and that the region's frequent mine accidents were caused not by geology or cost-paring operators who stinted on ventilation and other safety measures but by careless miners. The promoters were mistaken. However great the subsequent investment in the region's mines, however much miners and mine operators cooperated to make the business pay, whatever the protection afforded by tariffs, and whatever the efforts to convince miners to observe safe work practices, inadequate profits and catastrophic accidents plagued the industry until the mines finally closed.

In tracing these developments, Wallace effectively combines economic, social, ethnic, technological, labor, and even political history. He once again reminds us by example that the analysis of a single aspect of a problem, however detailed and

well done, does not begin to explain the whole. Life, motivation, and the interplay of people and forces even in the simplest communities are extremely complex. He also demonstrates that it is possible to employ the statistical tools and the concepts of social scientists without surrendering to technical jargon or giving up either good narrative or literary quality.

GERALD G. EGGERT
Pennsylvania State University

RONALD L. NUMBERS and JONATHAN M. BUTLER, editors. *The Disappointed: Millerism and Millenarianism in the Nineteenth Century*. (Religion in North America.) Bloomington: Indiana University Press. 1987. Pp. xxiv, 235. \$29.95.

William Miller's antebellum "adventism" has enjoyed a rich historiography for over one-half century, including both internal analyses of the movement (for example, Everett Dick's pioneering 1930 dissertation, "The Adventist Crisis of 1843-1844") and more general attempts to interpret Millerism against the background of American culture (such as E. S. Gaustad, ed., *The Rise of Adventism* [1974]). This recent addition to Indiana University Press's promising new series, Religion in North America, builds on and adds to that richness. Most of the book's eleven essays were first presented at a 1984 conference in Killington, Vermont, in honor of Vern Carner, a long-time promoter of Adventist history. Some of the authors are present or former Advent Christians or Seventh-day Adventists, but several have no connection with these two most significant religious groups to descend from Millerism. The book suffers from characteristic weaknesses of the genre: occasional repetitions, striking lacunae (especially the absence of a summary outlining the biblical exegesis that led Miller to his millennial conclusions), and a subtitle more accurate than the title (only a few of the papers and the appendix—moving, but brief, reminiscences from three antebellum adventists—directly concern the Millerites who were "disappointed" at the failure of Christ to appear on March 21 or October 22, 1844). On the whole, however, the essays are marked by painstaking research, broad focus, exhaustive scholarship, and forceful argument. The book, capably illustrated with material selected by James Nix, including a breathtaking twenty-two by thirty inch color facsimile of Joshua V. Himes's "Chronological Chart of the Visions of Daniel and John," must rank as one of the most successful symposia ever assembled for a subject in American religious history.

Several essays are primarily narrative. Wayne

Judd provides a sensitive sketch of Miller himself (1782-1849), the farmer from Low Hampton, New York, who interpreted the Bible to pinpoint the return of Christ "around 1843." David Arthur offers the same for Himes (1805-95), the communications genius who popularized Miller's views in something over five million pieces of literature and who was primarily responsible for the tens (perhaps even hundreds) of thousands who heeded Miller's message. Jonathan M. Butler puts anthropology to use in tracking the transformation of Miller's "boundless" biblical vision to the "consolidated" regimen of Ellen White's Seventh-day Adventists. Other essayists provide sharply focused monographs. Louis Billington shows how chiliastic Irvingites, Campbellites, and Plymouth Brethren in Great Britain were receptive to Millerism. Ronald L. Numbers and Janet Numbers offer an impressive interpretation of the once widespread belief that Millerism caused insanity (their conclusion: it was, and is, a badly posed question). Three essayists describe more particular relationships: Ronald Graybill on the way Millerites could favor, but not necessarily advance, abolition; Michael Barkun on John Humphrey Noyes's profound psychological antipathies to Millerism; and Lawrence Foster on why Shakerism became a halfway station for some Millerites en route to Seventh-day Adventism.

For historians of American culture, the three most interesting essays will probably be those that directly address the relation of Millerism to antebellum northern society. David Rowe's careful study of 353 Millerites in upstate New York leads him to conclude that Millerism was a mainstream movement of transplanted Yankee pietists. Eric Anderson, to the contrary, argues that "taming Miller and his movement" has gone far enough (p. 89); he wants to make it clear that Seventh-day Adventists, who believe that Miller's predictions did in fact come true spiritually (if not visibly) in 1844, have the most at stake in taking the sting out of the scandal of Millerism. Ruth Doan's essay is perhaps the most engaging in the book, with its suggestion that, although Millerism did arise from a standard antebellum mix of biblicism, revivalism, and millennialism, the very intensity of Miller's prophetic supernaturalism drove his more conventionally evangelical opponents toward increasingly nationalistic, immanentistic, and liberal forms of religion.

MARK A. NOLL
Wheaton College
Wheaton, Illinois

RUTH ALDEN DOAN. *The Miller Heresy, Millennialism, and American Culture*. Philadelphia: Temple University Press. 1987. Pp. xi, 286. \$34.95.

Adventism and millennialism in America have engaged renewed historical attention in recent years, and Ruth Alden Doan adds to this growing literature with an interpretive essay based on primary sources and secondary works. Her book presupposes familiarity with events treated in such recent works as David T. Arthur's doctoral dissertation on the Millerites (1970), Edwin S. Gaustad's edition of *The Rise of Adventism: Religion and Society in Mid-Nineteenth-Century America* (1974), and David L. Rowe's *Thunder and Trumpets: Millerites and Dissenting Religion in Upstate New York, 1800–1850* (1985).

According to Doan, a radical supernaturalism was the distinguishing characteristic of the Miller heresy. A literal reading of the whole Bible led Miller and his followers, good evangelical Protestants located primarily in the northeastern United States, to stress belief in a transcendent and powerful God and the second coming of Christ. This complex of assumptions about the "advent near" was the central tenet of the movement from its beginnings in the 1830s to 1843. Scriptural authority inspired claims to special knowledge and a vision of the final days, and Millerites developed a method for computing the precise time of the personal return of Christ. The setting of the time was not originally a part of the doctrine. Millerites met increasing intolerance as they made a precise date an integral part of their beliefs.

Doan's most original contribution is an exploration of Millerite dissent in relation to the process of change in American culture. Opponents feared that the Miller heresy undermined the developing ideal of character (as opposed to piety), the discipline of work, and the family. They viewed Millerites as mentally "unbalanced" and often confined them to insane asylums, a new institution at the time.

As Doan argues, the rejection of Millerism reinforced broad changes in American culture, the most important of which was secularization. Advancing her point, she describes the decline of church authority and the "domestication" of revivalism while stressing the differences between Millerite millennialism and the premillennialism of the period. In passing, she compares Millerites with other marginal groups victimized by the mainstream society (especially abolitionists and Mormons) in order to describe the emergence of a new cultural orthodoxy in the United States.

Doan's book is provocative, but nevertheless it troubles me in important ways. She treats various issues with little apparent appreciation of their historical dimension. Work discipline will serve as an example. She often imposes contemporary concepts or categories on the evidence in ways that strike me as hazardous. She generalizes broadly,

resorts to conjecture, and makes unwarranted emphases in arguing central points. She exaggerates the fear of deism, for example, and overstates the prevalence of divine immanence in the American culture of the period. At times, on vital interpretations, she is confusing and unconvincing. Her treatment of the Millerite contribution to secularization in America is a case in point. The confusion is compounded by a regrettably large number of errors of various types careful proof-reading should have eliminated.

WINTON U. SOLBERG
University of Illinois

MARIA DIEDRICH. *Ausbruch aus der Knechtschaft: Das amerikanische Slave Narrative zwischen Unabhängigkeitserklärung und Bürgerkrieg*. Stuttgart: Steiner. 1986. Pp. 310. DM 66.

This severe monograph of three hundred tight pages breaks down the literary and ideological ingredients of slave narratives as texts; it does not compose a picture of what slavery was like according to these texts. Maria Diedrich, author of *Kommunismus im afroamerikanischen Roman: Das Verhältnis afroamerikanischer Schriftsteller zur Kommunistischen Partei der USA zwischen den Weltkriegen* (1979), investigates the form as well as the function and contents of this peculiarly American literary genre. As a W. E. B. Du Bois fellow at Harvard, she did so with access to the full range of existing literary and historical studies of this stirring type of hybrid text—autobiography *cum* anti-slavery propaganda. The thirty-five autobiographies of former slaves that were published in book form in the United States between 1789 and 1861 and are recognized as authentic constitute the source material.

Diedrich rejects the separation of "literary-aesthetic evaluation and sociohistorical analysis" in interpreting these texts because their "propagandistic intention and political function" were "mediated or conveyed" (*vermittelt*) by the literary form of autobiography (p. 17). The expectations and literary tastes of their white readership as well as the will to assert the indestructibility of human dignity and equality guided the writers' or ghost writers' hands.

The substantive chapters are organized around five topics: the former slaves' view of human nature (chap. 3), the principle of equality (chap. 4), life and liberty (chap. 5), the pursuit of happiness (chap. 6), and the right to resist (chap. 7). Each of these chapters casts a very wide net into the ocean of Western civilization since Sophocles and then places relevant utterances and attitudes found in slave narratives in this context. The

chapter on equality, to take one example, progresses from a discussion of racism in colonial America and in the northern and southern states (based on standard historical monographs and references to Ernst Bloch, György Lukács, Max Horkheimer, Theodor W. Adorno, and Ralph Ellison) to the public debate about black inferiority in the eighteenth and nineteenth centuries, to demonstrations of black equality. The last-named section consists of the subheadings "Love," "Couples and Families," "Slaves as Husbands and Fathers," "The Role of Black Women," "Equality of Opportunity," and "Social Hierarchy." We are eleven pages into this chapter before the first slave narrative is quoted to make the point that the black narrator himself was occasionally shackled by racially stereotyped thinking and seduced to use animal metaphors in descriptions of black women ("lithe cat," "a brute's intelligence" [p. 98]). Five more phrases from three more slave narratives about the beauty of white skin and the spirit of freedom in white blood round off the picture of "internalized racist prejudices" (p. 99). Surprisingly, even in the following section on love in the slave narratives, psychohistory plays no role.

Throughout, the thirty-five slave narratives are considered all together as one great text to be explicated. This is done by pulling together statements from all of them on the series of major topics signaled by the chapter headings. The exegesis largely consists of pointing out literary associations (the reader "is reminded of David Copperfield," "remembers Milton's characterization of Eve" [p. 111]) and by listing a multitude of references across centuries of the history of ideas. Within the two and one-half pages on property owning, for instance, references flit from Aristotle to the Sermon on the Mount to Calvin to Rousseau to Swabian Pietists to Fichte to Marxian influences on Jack London to Helvetius to Hume to Hutcheson to Lord Kames to Adam Smith to Jefferson. Brief quotations from five of the slave narratives are stitched in between (pp. 223–25).

In the end the brilliance and richness of intuitive associations and references turns into cumbersome overload, even for the historian of antebellum American society who is quite willing to follow and learn from a literary guide through the treacherously simple slave narratives. There is no real summary to these essayistic chapters. Chapter 2, "New Evaluation of the Slave Narrative," is a sophisticated introduction to the genre and probably the most worthwhile part of the book for the historian.

The unrelenting academic prose is given touches of life and meaning on many a page by the quotations it sets out to explain. The authoritative eight-page selective bibliography on slave narra-

tives abbreviates first names, so that you may flip some catalogue cards before you find your F. Davis, J. Harris, and R. Miller (or is it P. Miller?). There is no general index but an index of names and titles.

WILLI PAUL ADAMS
Freie Universität Berlin

JOHN D'ENTREMONT. *Southern Emancipator: Moncure Conway, the American Years, 1832–1865*. New York: Oxford University Press. 1987. Pp. xiv, 282. \$29.95.

John d'Entremont recounts the first thirty-three years of the life of Daniel Moncure Conway, one of the foremost radicals of the antebellum South. Arguing that one can learn a great deal about institutions by studying those who could not adjust to their requirements, d'Entremont uses Conway's life to make valuable observations about the southern family, organized religion, and the antislavery movement. Descended from distinguished and wealthy Virginia clans, Conway was raised to assume a leading place in the slaveholding aristocracy. From an early age, however, he found himself unable to conform to the expectations of his family and society. First, Conway abandoned the study of law for the ministry, and then he left his family's Methodist faith for the almost exclusively northern Unitarian religion. Finally, during the sectional turmoils of the 1850s, he adopted an outspoken antislavery position that caused his virtual banishment from the South.

D'Entremont observes that, unlike most of the small number of prominent antislavery southerners, Conway opposed slavery for moral not economic or political reasons. The first abolitionist seeds were planted by his mother and aunts who taught Conway to question the aggression and domination that ruled southern life. Later, during his student years at Harvard, the acceptance and friendship that Conway encountered in Massachusetts reform circles cemented his allegiance to abolitionism.

D'Entremont also uses Conway's life to shed new light on developments inside the Unitarian church. Heavily influenced by Ralph Waldo Emerson and German rationalism, Conway became an intellectual leader of what d'Entremont dubs "left-wing Unitarianism." By the late 1850s Conway's questioning of the infallibility of the Bible and the plausibility of the resurrection made him anathema to conservative Unitarians. They feared that Conway's extreme deviation from orthodox Christianity jeopardized the denomination's social respectability. In the 1860s conservatives restructured the denomination to oust radical opponents such as Conway.

The Civil War was a turning point in Conway's life as well as the nation's. In lectures and writings Conway supported emancipation, but he also urged mercy for the South. Because of his southern background and hatred of violence, the war's mounting casualties disillusioned Conway. D'Entremont believes that Conway left the United States on a pro-Union speaking tour of Great Britain in 1862 to distance himself from the war's misery.

In Britain Conway's war-weariness led him to make an unauthorized proposition to a Confederate diplomat that abolitionists would abandon their support for the war if the South emancipated its slaves. When this proposal was made public, virtually all abolitionists repudiated it and vilified Conway. Badly stung, Conway criticized the abolitionists for forgetting their original priority of emancipation and disregarding the death and destruction caused by the war. Conway, however, never abandoned the cause of black freedom and energetically wrote and lobbied on behalf of equal civil rights.

Deftly written, d'Entremont's work makes exemplary use of family correspondence to explain the motivation behind Conway's constantly evolving public positions. By his skillful use of psychological insight, d'Entremont successfully refutes the portrayal of Conway as a neurotic and unstable individual in Peter Walker's rigidly psychoanalytic *Moral Choices: Memory, Desire, and Imagination in Nineteenth-Century American Abolition* (1978). D'Entremont's work focuses only on Conway's American years, but the author intends a second volume to recount his subject's long European self-exile after the Civil War.

JOHN R. MCKIVIGAN
Yale University

JAMES L. HUSTON. *The Panic of 1857 and the Coming of the Civil War*. Baton Rouge: Louisiana State University Press. 1987. Pp. xviii, 315. \$32.50.

In several articles published in 1982 and 1983, particularly those appearing in *The Historian* and *Agricultural History*, James L. Huston offered some interesting revisions of the Panic of 1857. This book, however, adds little to the points introduced in those articles. Huston argues that the economic downturn in 1857 was the result of an oversupply of grain, probably related to the end of the Crimean War; that the relative harmful effects of the panic were regional, more seriously felt in the North than the South; that the North and South each took different lessons from the panic; and that the Republicans, based on their reading of the panic, emphasized the tariff successfully in the

election of 1860 to swing critical Pennsylvania into Abraham Lincoln's column. The panic, Huston contends, revived economic issues in the political debate that slavery had buried.

Huston is on more solid ground in his views of the causes of the panic than he is with the effects, but his interpretation still suffers from several problems. He does not explain that specie suspension did not always mean that banks quit trading in gold with their customers altogether; instead, they often discounted the notes of other banks for interbank transactions. Suspension certainly did not mean that a bank failed, as Huston implies (p. 17), and his statement that, when circulation decreased, the remaining notes were essentially "worthless" is simply wrong (p. 28). Bank note reporters of the day show, for example, most banks in Pennsylvania trading in December 1857 at ninety-seven cents on the dollar. Prices on notes from other states were comparable. Huston's contention that the Crimean War affected grain production, though logical, is backed by little evidence: Huston provides no analysis of European grain production statistics for 1856-58, no figures on the number of European farmers who returned to the fields after serving in the army, and no statistics on government grain purchases of the European belligerents. His work in this area could have profited from J. R. T. Hughes's *Fluctuations in Trade, Industry, and Finance* (1960).

As for Huston's ultimate conclusion that the recession led to voter shifts in Pennsylvania, thus having a critical impact on the election of 1860, he has a strange way of convincing the reader that the panic was important at all. Through the first one hundred fifty pages we learn that the effect of the panic on the elections in 1857 was "debatable" (p. 50) and that it "did not stimulate Southerners to create new justifications for slavery" (p. 81). The panic "did not spark any new theoretical thrusts on the labor question from protectionists" (p. 103), and the attention Congress paid to the problems raised by the panic was "brief and cursory" (p. 121). Moreover, the Panic of 1857 related "only tangentially" to the Kansas debate (p. 121), and the economic issues raised in the elections of 1858 "were not explicitly connected to the Panic or to the [subsequent] depression" (p. 142). Later, Huston contends that the recession had an impact on the northern congressional elections of 1858 but even then finds its influence "more limited than one might expect" (p. 151). Huston spends the next one hundred or so pages linking the Pennsylvania votes in 1860 to the tariff and the tariff, in turn, to the panic. It is a rather schizophrenic approach.

Where Huston has opportunities to examine truly interesting questions, he passes over them.

He notes that real wages did not change in the winter of 1857–58 because all prices dropped. But, instead of exploring the psychology of workers in reacting to nominal wage decreases during the panic, Huston ignores the issue. He similarly fails to relate problems in the coal industry to the troubles with the railroads, which he mentions later. In one instance he claims that contemporary business chroniclers understood the panic's causes (p. 34), and yet three pages later the reader finds that contemporaries believed bankers caused the panic (which, as he argues, is incorrect). He accepts the view of the day's legislators that regulation should have aimed at finding a "correct specie reserve ratio" (p. 47) implying that higher ratios were better, and yet recent research on banks in the 1850s has shown that the more stable banks—in the North and South—generally held lower reserve ratios. And, contrary to Huston's assertion, Democrats had deviated drastically from free-trade principles, with the most obvious being their unwillingness to let the market operate in the area of money and banking. Southern Democrats led the way in creating government monopoly banks.

In numerous instances Huston fails to examine recent research. There is no discussion of Arthur Rolnick and Warren Weber's series of articles that challenge the view that free banks were unstable or of James Ward's revisions of Merl Reed's outdated study of railroads. No mention is made of Robert Loewenberg's provocative pieces on George Fitzhugh, even though Fitzhugh himself receives six pages, and proslavery arguments are given considerably more. Huston's analysis of free trade and the labor theory would have benefited by Loewenberg's dissection of Stephen Pearl Andrew's cost principle. The omission of recent studies of southern banking also stands out.

It is doubtful that a number of pronouncements related to slavery and free trade cited by Huston were in fact directly traceable to the panic. Certainly, the ideas that emerged from Fitzhugh's doctrines had their origins well before 1857, and even when Fitzhugh is quoted from 1857 or 1858 the foundation of his views was laid much earlier. Indeed, Fitzhugh seems to pose several problems for Huston. On page 87, we learn that Fitzhugh's theories "met with southern repudiation," yet on page 96 Huston states that there "was a discernable movement toward the patriarchal principle" elaborated by the Virginian. Huston's discussion of Thomas Malthus could have profited by reference to Malthus's original works rather than Robert Heilbroner's rather biased interpretation of them. At any rate the chapter on economic thought and the panic leaves me unconvinced that

the depression had any significant effect on either the proslavery or the free-trade positions.

Reviewers more versed in political quantitative methods will have to judge the merits of chapter 9, in which Huston applies statistical correlation analysis to Pennsylvania voting in the presidential election of 1860. Even if Huston's findings are correct, however, he still only explains the change in voter behavior of a small group in a single state. In short this book reaffirms what most historians have thought all along, namely, that the minor recession that occurred in 1857 was a relatively insignificant event in American history.

LARRY SCHWEIKART
University of Dayton

WILLIAM SAFIRE. *Freedom*. Garden City, N.Y.: Doubleday. 1987. Pp. xxi, 1125. \$24.95.

Sexy and cerebral Anna Ella Carroll, whose "bosom was an asset she never hesitated to display," continued an adulterous relationship with Senator John C. Breckinridge at the very time that she was writing a pamphlet severely critical of his public stand on the secession crisis. True or false? Well, the pamphlet is real enough, but the coitus is figmental. William Safire's *Freedom* combines history and fiction into what is now often called "fictionalized history" to distinguish it from historical novels written in the classic mode of *Ivanhoe* and *The Three Musketeers*. The book's many characters are all historical figures, and its plot follows the historical record. Literary imagination is largely confined to the invention of thoughts, dialogue, and specific incidents. Although fictionalized treatments of the Lincoln presidency have previously been published by Honoré Wiltse Morrow and Gore Vidal, this volume is unique in at least one respect. The 982 pages of narrative, covering only nineteen months from late May 1861 to New Year's Day 1863, are followed by a 136-page "Underbook" identifying sources, distinguishing factual material from fiction, and commenting on problems of interpretation.

Like Morrow and Vidal before him, Safire wants his work to be read for pleasure and at the same time to be taken seriously as history. Indeed, he regards his methods as superior to those of conventional historical scholarship because, as he told an interviewer, "you can get at the truth in a novel without the hangups of a historian." In the light of this credo and the influence that the book is likely to have on popular understanding of the Civil War, it seems appropriate in a historical journal to acknowledge the attractions of *Freedom* as literary entertainment but appraise it as a contribution to history.

From that point of view, the first two pages of the first chapter are not an auspicious beginning. Safire miscalculates Roger B. Taney's year of birth, misstates his length of service on the Supreme Court, embraces the myth that the chief justice was at heart an antislavery man, calls Dred Scott a "runaway slave" (an error subsequently repeated several times), and offers this summary of the election of 1860: "By running for President as a Peace Democrat, Breckinridge had split the vote of War Democrat Stephen Douglas, helping Lincoln to win the presidency." One cannot but wonder just what kind of war Douglas is supposed to have been advocating in the presidential campaign. On later pages the reader is misinformed that Lincoln's first son died in infancy, that Andrew Jackson and his "Democratic successors" were Hamiltonian centralizers, that Martin Van Buren was the only vice president elected to the presidency, and that John C. Calhoun plotted secession in the early 1850s.

Mistakes of this sort are common, however, only when Safire dips back into the antebellum period. On the war years themselves, and especially on the politics of wartime Washington, he writes with a depth of knowledge gained from extensive study of Civil War literature. Desiring most of all to make the past come alive, he is particularly skillful at dramatizing the random complexity of historical circumstances and events, such as Lincoln's bedeviled relationship with General George McClellan and his tortuous progress toward emancipation. Safire's characterization of the man in the White House is cumulated from the sources and, on the whole, consistent with recent scholarship. His discussion of Lincoln in the back of the book, though rather heavily judgmental, is sophisticated in its grasp of the limitations within which Lincoln had to operate and sympathetic in its understanding of the uncertainties, importunities, and frustrations with which he was plagued.

Yet there are also places where causal explanation becomes primitive. For instance it is presented as historical fact that Carroll "almost singlehandedly" secured the American party nomination for Millard Fillmore in 1856 and that she alone designed the western strategy leading to Ulysses S. Grant's capture of Vicksburg in July 1863. Safire, although he acknowledges the dubious quality of "unsupported recollection," uses such material whenever it suits the needs of his narrative. Thus, he emphatically endorses Carroll's own inflated version of her achievements, and with fine dramatic effect he retails an unlikely story that Lincoln offered the presidency to New York Democratic leader Horatio Seymour on the condition that he wholeheartedly support the war.

Here the author's novelistic purpose has plainly affected his historical judgment.

In fictionalized history, error and misjudgment are hard to separate from deliberate falsification as a literary technique. For example several passages in *Freedom* add up to a vicious portrait of Henry Wilson as a contemptible sexual deviate who gives away an important military secret and thereby enables the Confederates to win the Battle of Bull Run. Safire, in his commentary, discusses the tenuous evidence implicating Wilson without clearly accepting or rejecting it. One therefore cannot tell how much of his characterization of the Massachusetts senator is questionable inference and how much is literary invention.

Hervey Allen once declared that the historical novelist "is under obligation to alter facts, circumstances, people, and even dates," in order to give the reader "as complete an illusion as possible of having lived in the past." In *Freedom*, Safire readily follows that rule and succeeds in creating such historical illusion, but he does so at a cost that will disturb anyone who has invested some belief in the integrity of the past.

DON E. FEHRENBACHER
Stanford University

WILLIAM GLENN ROBERTSON. *Back Door to Richmond: The Bermuda Hundred Campaign, April-June 1864*. Newark: University of Delaware Press or Associated University Presses, Cranbury, N.J. 1987. Pp. 284. \$38.50.

In the spring of 1864, Union Lieutenant General Ulysses S. Grant undertook a three-pronged offensive in Virginia. A Federal command under Franz Sigel tried to clear the fertile Shenandoah Valley. Grant wrestled with Robert E. Lee's army in northern Virginia, and from the southeast Benjamin Butler's Army of the James threatened Richmond. Nearly all attention has focused on the Grant-Lee struggle, and rightly so, but, as William Glenn Robertson points out in this study, Butler's advance on Richmond played a key role in Grant's strategy to crush the Confederates and capture Richmond.

As Grant had planned, Butler with forty thousand troops was to land and secure the valuable supply base at City Point and advance some twenty miles toward Richmond, destroying rail lines to prevent reinforcements from arriving to help the beleaguered Confederates. In addition Butler's expedition would draw troops from Lee's army and support Grant's command in the fall of the Confederate capital.

At first everything worked according to design. Union cavalry disrupted the Confederate rail and

communication lines, and Butler secured City Point without a casualty. But, once the Federals began their overland march to Richmond, the campaign began to deteriorate. Despite railroad problems, Confederate General P. G. T. Beauregard skillfully accumulated a force sufficient to discourage a rapid Federal advance and later gathered enough manpower to seize the initiative and strike a powerful blow against Butler's command. The Federals, stunned by the attack, fell back on City Point, and the threat to Richmond ended.

In the postwar years numerous Civil War participants attacked Butler for both his incompetence and his failure to occupy Petersburg when he had such a golden opportunity. As Robertson argues, these charges were excessively critical. Other Union officers were equally to blame for the campaign's failure, and at the time of Butler's advance Petersburg was of little importance, because the Federal army straddled the railroad from Petersburg to Richmond. It was only when both Grant's and Butler's campaigns proved unsuccessful and the Federals circled to the south of Richmond that Petersburg became so important.

Robertson places blame for the failure of the Bermuda Hundred Campaign on the Federal high command. Corps Commander Quincy Gillmore appears incompetent and lacking in aggressiveness, and the other corps commander, William F. Smith, never believed in the plan. Butler emerges as an officer strong on administrative skills and short on military experience. Butler had the character to see the expedition through to its success, but he lacked the military knowledge and had to rely too heavily on Smith. Federal generals, Robertson charges, still had not overcome what Lincoln called "the slows."

From the Confederate side Beauregard managed a difficult situation effectively, all the while battling with a confused chain of command that included the secretary of war, the Confederate president, and the president's military adviser, none of whom regarded Beauregard favorably. Robertson also indirectly attacks Jefferson Davis for becoming embroiled in petty squabbles and managing tactical details that did not warrant his attention.

Overall, this is a solid campaign study, well supported with some good maps.

JOSEPH T. GLATTHAAR
University of Houston

MARK J. STERN. *Society and Family Strategy: Erie County, New York, 1850-1920*. Albany: State University of New York Press. 1987. Pp. xiv, 172. Cloth \$39.50, paper \$12.95.

This brief volume interweaves the analysis of large samples of quantitative data on the fertility of women and the school attendance of older teenagers in this New York setting with discussions of arguments and results from the literatures of social history, historical demography, and social and demographic theory. Over this period in the history of Erie County, fertility declined substantially (except among the wives of laborers), the economy shifted from commercial to industrial, and the heavily German-born population became more heterogeneous in its national origins.

The work suffers from its brevity and from its cursory treatment of issues and evidence. Mark J. Stern does not explicitly define his key concept of "family strategy," an amorphous notion that for him includes both individual decisions and group norms. Differing family (usually meaning parental) strategies are invoked to account for fertility decline and for variation in fertility among classes, but these variations in fertility also provide the main evidence that strategies existed.

A focus on fewer theories and more attention to their empirical explication would have strengthened the interpretation. At one point Stern stresses the distinction between class and stratification, but he thereafter refers to individuals grouped by occupation as classes rather than as strata; there is no bridge across the conceptual leap. It would appear that petty proprietors numerically predominated in the "old" business class, but that category also included merchants and manufacturers, some of whom were rich. Certainly a distinction that is key to the interpretation needs all of the empirical elaboration possible.

Sometimes, the wide-ranging survey of the literature yields a careless reading. Stern uses the absence of a lag between the decline of fertility and the rise of schooling to refute an argument of John Caldwell, an influential recent writer on the fertility transition. Stern is accurately criticizing an argument that Caldwell makes for Third World fertility declines. Caldwell, however, has noted that these changes were simultaneous in Western societies, and it is not surprising that the sequence in Buffalo conforms to the Western pattern.

The discussion of quantitative procedures also is too limited. A simple reporting of the sample sizes for all of the cells in the tables would allow the reader to assess whether the changing fraction of older teenagers who did not live with employed fathers would affect the results for differentials in schooling. Similarly, the bias fallaciously introduced by comparing families with all or no older teenagers in school (table 4.4, p. 111) could be assessed if the reader knew the numbers in each category. The cumbersome procedure that standardizes child-woman ratios for the age of the wife

has several weaknesses, especially in its unnecessary limitation of the extent of multivariate statistical analyses.

Stern's avowed method is to combine "hard data" and "rash speculation," and the volume may possibly be judged a success within the confines of this methodology. A tremendous amount of labor went into gathering the samples from the national and state censuses for Erie County, and Stern speculates rashly. Missing are the intermediate steps that focus precisely on theoretical issues, that provide detail on the independent variables most crucial to the argument, and that thoroughly assess the quantitative evidence.

DANIEL SCOTT SMITH
*University of Illinois,
Chicago*

MAURY KLEIN. *Union Pacific: Birth of a Railroad, 1862-1893*. Garden City, N.Y.: Doubleday. 1987. Pp. xiii, 685. \$27.50.

This book is the first of a two-volume work on the history of the Union Pacific Railroad. Although the research and writing of the volume was funded by the railroad, it is not a "company history" since Maury Klein was given complete editorial freedom. The building of the Union Pacific in the years after the Civil War was a unique and difficult task. In the extensive construction of the 1850s, most railroads had been built in areas already populated, and the remainder were located no farther west than the edge of the frontier. The Union Pacific was built in an unpopulated region, and when construction started west of Omaha in the summer of 1865 no railroad had yet been built across Iowa. Thus, the builders of the Union Pacific were faced with a constant shortage of supplies, the danger of Indian attacks, and the prospect of only a modest local traffic once the line was in operation. Although the federal government was aiding the project with an eleven-million-acre land grant and generous loans of U.S. bonds, the Union Pacific often found Uncle Sam to be a difficult taskmaster, requiring frequent lobbying efforts back in Washington. Finally, the competition with the Central Pacific to lay down as much track as possible caused much shoddy workmanship and resulting delays in the final acceptance by the federal government.

Klein sees the early history of America's first transcontinental railroad as being divided into three periods: "The Challenge" (1862-69), "The Quest for Respectability" (1869-84), and "The Road to Hell" (1884-93). Complicating factors in the early years included planning of the project

during the crisis of the Civil War, the long distance—fifteen hundred miles or more—between company headquarters in New York City or Boston and the railroad in Nebraska or Wyoming, and the continuing lack of strong leadership. Doctor Thomas C. Durant—a man better known for his skill in stock market manipulation than for the practice of medicine—was the vice president and operating head of the Union Pacific but was clearly unfit for the position. The Ames brothers were major investors in the Union Pacific, but both were incapable of strong leadership. Oakes was a man of action but busy in Congress, and Oliver in Boston was too cautious to manage Durant. General Grenville M. Dodge, railroad promoter but on duty during the Civil War, by November 1863 had convinced Lincoln that the proper eastern terminal for the Union Pacific was in the vicinity of Council Bluffs or Omaha. The first ground was broken for the railroad at Omaha on December 2, 1863. After the war was over, Dodge was made chief engineer for the railroad. Silas Seymour, early appointed a consulting engineer by Durant and long on the Union Pacific payroll, got in everybody's way and was referred to as the "insulting engineer." Finally, the first rails were laid in Omaha on July 10, 1865, but little real progress was made until the Casement brothers, Daniel and Jack, were placed in charge of construction early in 1866. By September of that year 180 miles of track were laid, and by the first of 1867 the base construction camp was at North Platte, 293 miles west of Omaha. During 1867-68 another 450 miles were put down, and in the spring months of 1869 the advance grading crews of the Union Pacific and the Central Pacific, in their desire to lay as much subsidized line as possible, passed each other with parallel lines. On May 10, 1869, the Golden Spike was driven with Durant, Dodge, and assorted officials present. Rather surprisingly, no government officials from Washington were present for the ceremony.

Even after the Golden Spike millions of dollars were required to rebuild and upgrade the Union Pacific to governmental specifications. In the decade and one-half after 1869, the "Quest for Respectability" was difficult since financial problems, disputes with the government, and quarrels among those who had profited from the *Crédit Mobilier* all continued to plague the railroad. Tom Scott of the Pennsylvania Railroad controlled the Union Pacific briefly in the early 1870s, and after the Panic of 1877 Jay Gould controlled the road for several years. Some profits from land grant sales and newly opened western coal mines helped, but bitter, long-running rate wars with rival western railroads kept the Union Pacific from being profitable. Charles Francis Adams, Jr., was

president of the Union Pacific from 1884 until 1890 but was never successful with the reforms he had planned for the line. A poor judge of people, Adams was indecisive in action and lacking in self-confidence. His financial problems were deepened by the cost of adding hundreds of miles of branch lines to the system. The Union Pacific was in deep financial trouble by the early 1890s and was placed in receivership in 1893.

In this complex history of the Union Pacific, Klein includes enough excitement and suspense to make knowledgeable historians sometimes wonder how the story may end. The author concentrates on problems of management, finance, and politics and only briefly reviews labor and labor problems, traffic and revenue statistics, and the motive power and equipment needs of the railroads. The volume includes over one hundred historical photographs, but the index is limited in scope, and many readers might welcome additional maps. An ample 117-page pamphlet on source notes is available on request from the Union Pacific Museum in Omaha. Klein has written a detailed and definitive history of the early years of one of America's most important railroads.

JOHN F. STOVER
Purdue University

THOMAS R. TRAUTMANN. *Lewis Henry Morgan and the Invention of Kinship*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xiv, 290. \$30.00.

Lewis Henry Morgan's *Systems of Consanguinity and Affinity of the Human Family* (1871) may be considered the most significant anthropological work of the nineteenth century. It compiles a vast amount of data on terms of relationship, demonstrates that each set constitutes a system with its own unique logic, argues that similarities in the structure of kinship systems reveal remote historical relationships, and suggests that the patterns of marriage and descent that gave rise to these systems can be reconstructed to produce a social history of human society from earliest "savagery" to the present.

There have been many previous studies of Morgan, but none has taken Thomas R. Trautmann's tack of comparing in detail the surviving drafts of Morgan's magnum opus to demonstrate the development of his thought as he responded to the intellectual revolution of the 1860s that led to the collapse of the biblical chronology and the rise of social evolutionism as a scientifically constructed history of human progress. The author demonstrates that Morgan's concept of the human mind

remained constant throughout his lifetime. Morgan assumed that all human beings had the same mental capacity; differences between savages and civilized peoples were a matter of differential progress. Thus, in the uncivilized peoples of his own day Morgan envisioned the past of European civilization. The lengthening of the time span of human history, based on developments in geology, archaeology, and organic evolution, provided the expanded scale necessary to account for both the commonalities and differential in human progress.

Beginning with the realization that the matrilineal kinship reckoning of the Iroquois of New York formed a logical system, but one founded on different principles from that of Western civilization, Morgan at first decided that he had uncovered a unique phenomenon. His subsequent discovery that the Ojibwa of Michigan, linguistically unrelated to the Iroquois, shared the same structure of kin terminology, convinced him that he had chanced on a "new instrument" for ethnological study, one with the potential to succeed where comparative philology had failed: to demonstrate the unity of American Indian peoples. After investigating American Indian kin systems, Morgan broadened his scope to the world, and, when the same structure turned up in Tamil kin terminology, he considered his efforts vindicated, interpreting this as a demonstration of the Asiatic origin of the American Indian.

Trautmann details the process by which, partly at the insistence of his publisher, the Smithsonian Institution, Morgan generalized his historical conclusions to embrace an entire conjectural history of humanity, in the mold of the Scottish moral philosophers. Today Morgan's evolutionism is all but forgotten, but the principles of kin classification that he first elucidated are an important source of ongoing study and debate in anthropology.

Trautmann's book is a well-conceived historical study that sets Morgan's work firmly in the context of his times. Its weakness is a failure to appraise comprehensively the subsequent anthropological scholarship about Morgan and the uses to which his data and theories have been put. But this book is a solid foundation for further study, a valuable contribution that will interest anthropologists and historians alike.

RAYMOND J. DEMALLIE
Indiana University

NELL IRVIN PAINTER. *Standing at Armageddon: The United States, 1877-1919*. New York: W. W. Norton. 1987. Pp. xlv, 402. \$25.00.

Rather than drawing a line between the so-called Gilded Age and Progressive era, Nell Irvin Painter treats the period as a single continuum. Notwithstanding the obituaries proclaiming the death of progressive history, its Manichean approach to the American past is alive here—if not well. The book's thesis is that the politics of the time revolved around a conflict between the exponents of two rival visions of society: "One group spoke the persuasive idiom of prosperity; the other, the equally positive language of democracy. Advocates of prosperity prized order, which would issue from a hierarchical arrangement of society in which the more able few would make decisions for everyone. Advocates of democracy, on the other hand, reclaimed the right of each man to speak for himself . . . Champions of prosperity invoked an identity of interest among all members of society, be they owners or workers . . . Democratizers believed that society naturally entailed conflict, and in the conflict of interests they championed those of the disadvantaged" (pp. xxxix, xliii).

The reader is not left in doubt where Painter's sympathies lie—with those standing for "fair play, democracy, and openness" against "the tyrannies of wealth and conventionality" (p. 280). The struggle had two dimensions: one for "economic equity" (p. 282), the other to end the "oppression" (p. 365) suffered by ethnic and racial minorities and women. And she identifies "labor (in its broadest sense)" (p. xii)—that is, including farmer protest movements—as the vanguard in the battle for "fairness" (p. 253). Although she acknowledges that "the nativism, racism, and sexism that characterized the period appeared among the well-meaning" (p. xxxviii), that difficulty is not allowed to interfere with her argument. And if the reforms first advocated by "labor" gained substantial middle- and even upper-class support after the turn of the century the reason is simple. "Plain, stark fear . . . of working-class violence explains much of what has been called progressive reform" (p. xii).

Painter's coverage is as skewed as her interpretations. The constructive achievements of businessmen—as evidenced by her own statistics of economic growth—have no place in her account. Nor do the remarkable strides made in dealing with the sheer physical problems accompanying rapid city growth. Intellectual, literary, scientific, and educational developments receive scant attention. Foreign policy before the Wilson years is similarly slighted—and to the extent dealt with has as its motivation the quest for markets, protection of investments, or racism. Even as an exercise in the now-fashionable history from below, the book has major shortcomings. Painter's most important contribution is her rescue of blacks and women

from the neglect too typically their lot in accounts of the period. On the other hand, she not simply accepts at face value the grievances put forth by farmer protest movements but slides over how such movements attracted a fraction of even the farm population. And her treatment of the working class is almost exclusively focused on the more spectacular strikes and labor disturbances.

There is no bibliography. The footnotes are for the most part limited to quotations. Some are directly from contemporary sources (largely periodicals); the majority from published secondary works. One wonders how familiar Painter is with the latest scholarship. She exaggerates, for example, the extent of Supreme Court negativism toward regulatory legislation. She cites Maury Klein's *The Life and Legend of Jay Gould* (1986) but repeats the standard clichés about Gould as no more than a financial manipulator and despoiler. Although she alludes in a footnote to Paul Kleppner's findings about the ethnocultural basis of late nineteenth-century voting patterns, the accompanying text blithely generalizes that the "Democrats traditionally represented working people in industrial cities like New York and Chicago and white southerners . . . The Republican party represented farmers in the Midwest, workers in a few cities like Philadelphia and Cincinnati, and southern blacks" (p. 74).

The writing is pedestrian with more than its share of malapropisms. My favorite is how Andrew Carnegie (who is described as a "singular millionaire") "welcomed enormity in business" (p. 93).

In short, specialists will not find much of interest. The work's intended audience appears to be the undergraduate textbook market. But its appeal will be limited to those with a similar political axe to grind.

JOHN BRAEMAN
University of Nebraska

WORTH ROBERT MILLER. *Oklahoma Populism: A History of the People's Party in the Oklahoma Territory*. Norman: University of Oklahoma Press. 1987. Pp. xiii, 280. \$22.50.

The title of Worth Robert Miller's study of Oklahoma's Populist movement is deceptive because the author goes well beyond a simple analysis of the Oklahoma People's party. Indeed, Miller presents convincing evidence that Populism in the early years of Oklahoma Territory was a microcosm of the national movement. It was a fascinating amalgamation of midwestern wheat-belt farmers and southern cotton farmers that managed to attract as much as 33 percent of the vote during the race for territorial delegate in 1894.

From a narrative standpoint, Miller's study has one great advantage. Never did the chronology of a state start with such dramatic abruptness. On April 22, 1889, somewhere in excess of fifty thousand non-Indian settlers rushed across the plains of central Oklahoma to claim two million acres of so-called Unassigned Lands. Virtually overnight, towns such as Guthrie, Norman, and Oklahoma City materialized on what was an almost untouched carpet of grass sprawling as far as the eye could see. A majority of northerners were from Kansas, while most of those from Dixie were Texans; the Canadian River soon became the main dividing line. The most prominent leaders of the newly arrived Populists, however, tended to be members of an egalitarian Kansas movement who had struggled for identity and power long before coming to Oklahoma.

Proponents of Oklahoma's People's party strongly identified with the "republican ideology" of their founders. They focused on the importance of maintaining liberty and virtue in the face of the successful business classes that acquired so much strength and power during the Gilded Age. In fact Miller describes these elitist groups, most of whom had the cash to reach Oklahoma by railroad car rather than horse or wagon, as cosmopolitans whose panaceas were "laissez-faire capitalism, social Darwinism, and business boosterism" (p. 8). These particular Boomers, as a matter of fact, were inclined to grab town sites at the railheads rather than good farm land. While they dreamed of making handsome fortunes in a world that rewarded the most enterprising, their victims among the young People's party were usually farmers who righteously wanted to remain a part of America's long-revered Jeffersonian tradition.

Miller effectively traces the difficult course of the new party, which rose rapidly during the territorial years only to give way to socialism as a protest movement in Oklahoma a few years after the controversial fusion of Populists with Democrats. Regarding the overall movement as one that could have displaced the Democrats nationally if Grover Cleveland had retained party control by 1896, Miller shows considerable sympathy for Oklahoma's confident Populists. If any criticism could be found of his treatment of this third party, it would be the rather striking ideological bent he perceives in most Populist voters. His research is also impressive; the quantitative tables in the back of the book, for instance, undoubtedly strengthen his conclusions regarding electoral matters in the young territory.

ROBERT W. LARSON
University of Northern Colorado

JOAN M. JENSEN. *Passage from India: Asian Indian Immigrants in North America*. New Haven: Yale University Press. 1988. Pp. x, 350. \$32.50.

Joan M. Jensen's study deals with Asian-Indian immigrants in the United States and Canada from the arrival of the first Indian migrants in the late nineteenth century through World War II. These included students, political exiles, and those who worked in the western provinces and states. By discussing both Canada and the United States, she enables the reader to see a broader picture than could be obtained by simply considering one country. Because of the similarities in the Indian experience in these countries, it is essential to tell us of these immigrants in both nations and how non-Indian North Americans responded to Asian Indians.

The bulk of her story deals with the nativist response to Asian-Indian immigration. Her book skillfully explores the violent and legal hostility that developed in the North American West, for both white Canadians and Americans had little use for Asian Indians. Jensen makes it clear that Indians were considered another undesirable Asian group that was eventually banned from the United States and Canada. Moreover, white Americans and Canadians cooperated with one another to keep Indians from their shores. Indians did have some friends, but they were not many and were usually limited to employers who wanted a steady supply of cheap laborers.

The book is the product of a great deal of research in private and governmental archives. Jensen narrates not only the details of the campaigns to exclude Indians but also subsequent governmental efforts to spy on and deport these Asians, who were considered supporters of Germany in World War I or potential radicals threatening British rule of India. It is a sad story of colonialism and racism among white Canadians and Americans.

Once exclusion became a reality, first in Canada and then in the United States, the U.S. government pursued a policy of attempting to denaturalize the few Indians who had become American citizens before the meaning of "white" in the naturalization laws was clarified. This effort only ceased when the government lost in the courts, and not until after a number of Indian citizens had lost their naturalization.

Though thorough and well-researched on the anti-Indian movements, the book tells us less about the ten thousand or so immigrants themselves. Jensen does describe the first immigrants, who they were, and how they fared in their adopted lands. They took what jobs they could find, mostly in agriculture and lumbering. A small

number studied in the United States. Although not opposed on economic grounds, these students sometimes were viewed as political radicals and troublemakers. Her discussions of Asian-Indian life, though informative and interesting, are less detailed than the excellent treatment of the anti-Indian movements. It would be unfair to criticize Jensen for not writing another book. The reader should be informed, however, that the strength of this solid work is on hostility to Asian Indians and not the immigrants themselves.

DAVID M. REIMERS
New York University

SARAH DEUTSCH. *No Separate Refuge: Culture, Class, and Gender on an Anglo-Hispanic Frontier in the American Southwest, 1880-1940*. New York: Oxford University Press. 1987. Pp. vi, 356. \$34.50.

In this pacesetting book, Sarah Deutsch brings social history, women's history, Chicano history, labor history, and western American history into a logical, long-needed convergence. Exploring the development and decline of a "regional community" of Hispanic people extending from northern New Mexico into Colorado, Deutsch makes a persuasive case for the central significance of women in the processes of conquest, economic and cultural subordination, and social change.

Defying any stereotypes of a "passive" Hispanic character or a "completed" Anglo conquest, northern New Mexico villagers after 1880 responded to greater pressure on local resources with a new strategy: men traveled to supplement village income with waged work in railroad construction and maintenance and in coal mining. In many ways this "strategy" worked, bringing necessary income—and thereby a kind of independence—to the villages, keeping the men tied to their homes, and expanding the role of women as maintainers of the community's core. Often caring for the fields in addition to the homes and gardens, presiding both symbolically and actually over the preparation and distribution of food, women held the center together while men journeyed back and forth from the periphery.

When families moved along with men, the price of relocation escalated. In coal camps or in the substandard housing of Colorado sugar beet fields, the position of Hispanics in general and Hispanic women in particular deteriorated. As families moved geographically away from their center in New Mexico toward the margins of their region, the position of women underwent a parallel relocation from center to periphery, as they faced life without their gardens, their extended families, and their status as the permanent, often

property-holding residents of the home villages. The "regional community" foundered without its anchors.

Although Deutsch offers a lively critique of Anglo antimodernists determined to ensure that Hispanic villagers would embody the proper simple and natural virtues, one has moments of wondering whether a streak of antimodernism might be at work in both the book's author and the sympathetic reader. The villages and their strategy of community maintenance through male migration can sometimes seem improbably harmonious. A few brief references to a high rate of infant mortality suggest one way in which village life was apparently less than ideal. Similarly, the use of the word "autonomy" to describe what the villages had and lost seems to set the standards very high, especially for people undergoing conquest.

In matters of Hispanic-Anglo interaction, however, the book packs a particular punch, breaking through any spell of ethnographic harmony. Exploring the conversion efforts of Anglo women in the "home mission" field, the assimilation efforts of the "Sociology Department" of the Colorado Fuel and Iron Company, or the purifying efforts of the antimaterialist Anglos devoted to "authentic" Spanish colonial arts and crafts, Deutsch gives a dynamic portrait of Anglo expectations meeting Hispanic actualities. Here, too, she makes a convincing argument for the centrality of women as the staffers of the front line of assimilation and accommodation on both the Hispanic and Anglo sides.

With exhaustive research in many sources, Deutsch has produced an important study, one of the first major efforts in western American history to give "culture, class, and gender" each its due.

PATRICIA NELSON LIMERICK
University of Colorado

CHARLES DEBENEDETTI, editor. *Peace Heroes in Twentieth-Century America*. Bloomington: Indiana University Press. 1986. Pp. 276. \$22.50.

This book is another attempt by scholars to identify courageous and principled peace activists. The editor, Charles DeBenedetti, rescues nine peace heroes, "citizen peace leaders," from being lost to public memory. He offers, in what proved to be his final publication (his death from a brain tumor prematurely ended a productive career as a specialist in peace studies and American diplomatic history), an especially perceptive and informed introductory essay on the twentieth-century American peace movement. The book is appropriately dedicated to Bernice Nichols, long-time director of the Swarthmore College Peace Collec-

tion; the authors of the essays are active members of the Council on Peace Research in History.

Jane Addams, the only woman among the nine subjects, typified most of the others by her involvement in numerous reforms, but Michael A. Lutzker correctly emphasizes her peace work with the Woman's Peace party. Excellent biographical essays are also presented by the other leading peace movement scholars. Lawrence S. Wittner's essay on Eugene Debs credits him with a pioneer role in the Socialists' "struggle for a just society" (p. 81) and places him in the war resistance tradition in America. Moreover, Debs was certain that peace work among "ordinary people" offered the best hope for world peace. Charles Chatfield emphasizes Socialist leader Norman Thomas's activities as a dissenter whose primary goal was peace and whose primary motivation was the Social Gospel. Harold Josephson probably had the most challenging subject, Albert Einstein, who was selected to represent scholars among this century's peace seekers. Einstein frequently spoke and wrote in the interwar decades on issues of war and peace and on civil liberty and Zionism. He was an advocate of alternative nonmilitary service and war resistance, and, after 1945, he worked as chair of the Emergency Committee of Atomic Scientists and on behalf of world government.

Jo Ann Robinson uniquely presents her subject, citizen peace "prophet" A. J. Muste, in a brief biographical introduction followed by a longer interpretive section, an organizational device that might have been of value to others. This results in the shortest essay but one that is sympathetic, well written, and thorough. No one in the present-day peace movement can read this essay without acknowledging, some perhaps for the first time, the significance of Muste's thinking and example for his day and ours.

More familiar peace heroes are the subjects of Milton Katz (Norman Cousins) and Ralph B. Levering (Martin Luther King, Jr.). These writers emphasize the peace views and activities of Cousins and King in the 1950s and 1960s, stressing Cousins's role in the work of the National Committee for a Sane Nuclear Policy and King's "inclusive peacemaking." The emphasis in these essays is also on their subjects' influences on public policy.

Anne Klejment presents Daniel Berrigan and Philip Berrigan as "moral witnesses" and as symbols of recent Christian nonviolent revolutionaries. Because the Berrigans were Catholic priests, this essay best examines the peace views and activities of American Catholics in the years since 1960, emphasizing the acts of radical civil disobedience by the Berrigans and others.

Merle Curti, dean of American scholars of the

peace movement, concludes the book with a brief essay placing these nine peace leaders in the context of the "American heroic tradition" and discussing common motivations and experiences with great understanding. Curti, speaking for the other contributors, also urges that we not dismiss these peace leaders as subversives or troublemakers. Nor should we today regard "citizen-peace heroism" as no longer needed. That sentiment echoes DeBenedetti's introductory plea for "citizen peace activists" to "displace the prevailing myth of peace through superior armed strength with some practical understanding of security in disarmament and in the non-violent multilateral resolution of disputes" (p. 19).

The bibliographies and notes in this work, as well as the informed and well-written essays, make this book attractive for classroom use in history courses. Moreover, recalling the commitment, the words, and the actions of these nine peace heroes may even stimulate further citizen peace activity on the part of the readers.

ERNEST C. BOLT, JR.
University of Richmond

ROALD F. CAMPBELL *et al.* *A History of Thought and Practice in Educational Administration*. New York: Teachers College Press. 1987. Pp. xii, 241. Cloth \$27.95, paper \$16.95.

Whenever the first history—or even one of the first histories—of a significant and newly developing area of inquiry appears, it is likely to be more than normally noteworthy but less than definitive. This volume by Roald F. Campbell, Thomas Fleming, L. Jackson Newell, and John W. Bension fits that pattern. Neither the history of education nor the history of management lacks a comprehensive bibliography; likewise, there exists an abundant literature on educational management. This book, however, identifies itself as the first broadly based history of educational administration. As such, students of educational administration should welcome it. They should temper their expectations of it, however, with the realization that it presents its subject in a very uneven manner.

The title suggests a more comprehensive book than the four authors actually present. They focus totally on the American experience, almost exclusively on the twentieth century, and largely on the public schools (one of the best parts of the book, however, discusses college and university governance). They give major—probably excessive—attention to contextual information, including the social history and psychological, business management, and organizational theories that directly influenced educational thinking and planning.

Most notably, the book is long on theory and short on practice; there is much on the evolving philosophies of school governance but very little on how administrators have operated their schools on a day-by-day basis. The authors ascribe this imbalance to a lack of available source material: "objective descriptions of administrative practices . . . are relatively scarce" (p. 125).

Perhaps this problem of inadequate data to include in the section on administrative practice explains why the authors awkwardly forced into this section the chapter on the graduate training of educational administrators and the development of educational administration as a professional field. This useful chapter discusses the pioneering contributions of scholars Ellwood P. Cubberley and George D. Strayer in the early twentieth century, the emergence of educational administration as a major academic and professional unit after World War II, and the early and continuing role of Teachers College, Columbia University.

The next scholar or scholars to write a history of educational administration in America will, I hope, have more and better data with which to work. Perhaps Teachers College could provide the needed source material by employing the effective services and techniques of the Oral History Research Office of the university to collect extensive data from retired administrators, teachers, and board members whose experience spans those earlier years that offer few written records.

WILLIAM C. RINGENBERG
Taylor University

KEITH C. PETERSEN. *Company Town: Potlatch, Idaho, and the Potlatch Lumber Company*. Pullman: Washington State University Press or Latah County Historical Society, Moscow, Idaho. Pp. xix, 284. Cloth \$25.00, paper \$15.95.

The history of lumbering in the interior Pacific Northwest is only now being explored, gingerly, as separate from the forest industry of the Washington and Oregon coasts. The pine industry of the interior is overshadowed by the Weyerhaeuser syndicate and is comparatively small. Hence, it has been neglected.

Keith C. Petersen illuminates a segment of the Weyerhaeuser enterprise, the construction of a huge Idaho mill in 1906, perhaps the last big steam-driven mill in the United States, and of a company town for employees and managers, modeled after Pullman, Illinois. Potlatch Lumber and Potlatch, Idaho, have appeared in print before with broader strokes, but company towns, in contrast to company-dominated towns, are uncommon in the interior Northwest. Petersen's discus-

sion of the town is detailed and, for the most part, fresh.

The mill was dismantled in 1981; the town, released from company ownership in 1952, lives. Petersen, who calls himself a "non-affiliated public historian" (p. xi), walked through it, photographed it, talked to old residents, and helped persuade Potlatch Corporation, successor to the original company, to give its papers to the University of Idaho. He knows the town and captures the fondness for Potlatch so evident in recollections of old residents.

Roughly the first third of Petersen's narrative covers the migration of Weyerhaeuser from the Great Lakes to Idaho and construction of the mill under the direction of a colorful visionary, William Deary, who was primarily responsible for its location and design. The tragedy of the mill was that its lumber from prime timber was poorly cut and hard to sell. The rest of the book describes the town, socially stratified and maintained by a town-site department that painted the houses, tried to keep the place quiet at night, repaired utilities, and even ran a "honey wagon" to empty drawers of outhouses (p. 138). Yet, "while the company did want an ideal community, its shareholders were unwilling to sacrifice profits," so the company profited from Potlatch (p. 138). This theme of company primacy surfaces elsewhere in the narrative.

Petersen writes in a workmanlike manner, although he does not satisfactorily overcome the bugbear of profiling his actors without interrupting the narrative, and he could have condensed his account of Weyerhaeuser operations before moving to Idaho, since this is available in Ralph W. Hidy, Frank Ernest Hill, and Allan Nevins's *Timber and Men: The Weyerhaeuser Story* (1963). He doubtless has written the book others will rely on for information about Potlatch. A community effort of contributors who supported writing and publication, this book shows what dedicated activists can do to document the hometown past.

Petersen is not analytic, and his topic is limited. He does, however, sum up the Potlatch venture as a failure from the stockholders' view. He has contributed another component of a needed synthesis of the inland pine industry and Weyerhaeuser's place in it.

JOHN FAHEY
Eastern Washington University

JEFFREY T. SAMMONS. *Beyond the Ring: The Role of Boxing in American Society*. (Sport and Society.) Urbana: University of Illinois Press. 1988. Pp. xix, 318. \$24.95.

Concentrating on the heavyweight division (but frequently referring to nonheavyweights such as Ray Robinson, Battling Siki, and Jake LaMotta), Jeffrey T. Sammons endeavors to "focus on the contact points between boxing and societal developments" (p. xiv). Most of his book has to do with the period since World War I, although he includes substantial chapters on the emergence of prize fighting in the eighteenth and nineteenth centuries and the explosive career of Jack Johnson. From the redrawing of the color line by Jack Dempsey and Gene Tunney and the widespread public acceptance of boxing in the 1920s, Sammons moves on to describe boxing's decline and then revival in the 1930s with the emergence of Joe Louis. He then treats the interrelated effects of television and organized crime in the post-World War II years and the rise of Muhammad Ali as a wholly new force in and out of the ring.

Sammons gives a great deal of attention to the historical relationship between race and prize fighting. But he is also much concerned with boxing as a marginally criminal activity and with the symbolic importance of the various heavyweight champions (as well as many challengers). Finally, he seeks to explain why, despite more than a century of moral and medical condemnation and protest, boxing has maintained its hold on a broad segment of the sports-loving public. His answers have to do with the manipulation (often by criminal elements) of people's violent impulses, with the mythical values reinforced when "men rise above all the muck and emerge as knights in shining armor" (p. 257), and with prize fighting (and perhaps sports in general) as a means of social control. Boxing, Sammons argues, is part of the "diversionary or deflective strategies" that the "system" employs to keep fans' attention away from "problems that have been obscured by the successes and failures they have shared vicariously." Boxing thus becomes another instance of "the masses" aping the tastes and values of the privileged classes—"those who have the time and money for leisure, recreation, and spectator sports" (p. 257).

On that attenuated Veblenian and neo-Marxian note, Sammons ends a fascinating if also seriously flawed book. Often mechanically off base (dangling modifiers, disagreements of subject and verb, misspellings, who-whom confusions, trite phrasings), his book sometimes misses the mark factually as well. For example the \$81,500 that exchanged hands before the 1923 Dempsey-Firpo match was Tex Rickard's rent on the Polo Grounds (on which Francis X. McQuade sought a 10 percent commission) not a bribe paid to chairman William J. McCormack of the boxing licensing commission (pp. 69–70). Sammons surely

knows better than to assert that "southerners did not fully realize the social and economic values of successful athletes until the 1970s" (p. 104). And what is one to make of the statement, from the perspective of 1978, that Madison Square Garden had been "long a place where blacks could not even fight" (p. 225)?

Thoroughly researched, written with conviction and verve (if occasionally with hyperbole), the book is particularly strong on the post-World War II boxing scene. Sammons offers a fine account of the sinister role of Frankie Carbo and remarkable insights into the character and careers of Floyd Patterson, Sonny Liston, and, especially, Ali. With a good deal of rethinking and tightening and closer editing, his useful and provocative book could have become a much better one.

CHARLES C. ALEXANDER
Ohio University

MELVYN DUBOFSKY. *"Big Bill" Haywood*. New York: St. Martin's. 1987. Pp. viii, 184. \$19.95.

William D. Haywood, the best-known leader of the Industrial Workers of the World (IWW), was an enigmatic figure to his contemporaries and has been equally illusive to historians. In this brief biography, first published by the Manchester University Press for a British audience, Melvyn Dubofsky attempts to "measure the character" of Haywood's life and "his contributions to the society which bred him" (p. 41). Using new sources made available after the publication of his *We Shall Be All* (1969), Dubofsky has sought to uncover the "real Big Bill" (p. 6).

The body of this work is a straightforward account of Haywood's life and work from his youth in the mining West to his years of exile in the Soviet Union. Successive chapters trace his journey from Western Federation of Miners trade unionism to labor radicalism, from socialism to syndicalism and, finally, to Communism. There is little new here, and at times the treatment seems more a survey of the IWW than one firmly centered on Haywood. It is only in the concluding chapter that Dubofsky comes to grips with his subject and offers conclusions about "a life riddled with inconsistencies and contradictions" (p. 141). In this he is at his best in delineating the contradictions in the man: he is less successful in bringing coherence to Haywood's life. "Big Bill" remains a man of intriguing opposites.

This is a sympathetic biography that depicts Haywood as "an American original" (p. 114) whose radicalism evolved from his nation's past rather than from socialist or syndicalist ideology. His tragedy was that, in striving to make his native

land live up to its ideals, he became "the most feared man in America"; forced to live his last years in lonely exile, Haywood died "an alien both from his native land and from his host Soviet society" (p. 137).

Dubofsky includes a short essay on sources that is a useful introduction to the IWW and its charismatic leader; he also provides appendixes that include union documents, labor songs, and Haywood's last letter. Less helpful to the novice is the disconcerting lack of attribution of some quoted material in the text. Readers who wish to explore these points more fully or wish to know the source of some quotations will search in vain in the notes.

This book can be recommended to those who wish a brief account of a fascinating life. It is, however, no substitute for Joseph Conlin's *Big Bill Haywood and the Radical Union Movement* (1969) or the fine biography by Peter Carlson, *Roughneck: The Life and Times of Big Bill Haywood* (1983).

ANNE HUBER TRIPP
Oakland University

JANICE R. MACKINNON and STEPHEN R. MACKINNON.
Agnes Smedley: The Life and Times of an American Radical. Berkeley and Los Angeles: University of California Press. 1988. Pp. xi, 425. \$25.00.

Agnes Smedley is not a major figure in U.S. history textbooks. She cannot easily be turned into a familiar figure of American reform. She was, to her death, a forceful supporter of anti-imperialist movements and international revolution and a caustic critic of the institution of marriage.

Smedley grew up in poverty in rural Missouri and Colorado at the turn of the twentieth century. Active with the Socialist party in San Diego and New York City, she became involved in the birth control movement and became a close friend of Margaret Sanger. Her interest turned toward anti-imperialism, in particular the cause of Indian nationalism, arousing the hostility of the British government and resulting in her arrest by the U.S. government during World War I.

She continued these activities and a stormy personal relationship with the Indian nationalist Virendranath Chattopadhyaya in Germany in the 1920s before finally turning to her life's work in China. There she quickly identified with the Chinese peasants, nationalists, and revolutionaries. In articles and books she informed the world of Chinese resistance to Japanese imperialism; she traveled with the Red Army and raised money for medical supplies. Returning to the United States, Smedley attempted to educate Americans about China and the Chinese.

Janice R. MacKinnon and Stephen R. MacKinnon have drawn on their expertise in twentieth-century Chinese history to present a well-written, extensively researched portrait and analysis. This monograph should contribute substantially to the interest raised by the republication by the Feminist Press of Smedley's autobiographical novel, *Daughter of Earth*. Their sources include widely scattered material from China, Great Britain, and the United States, Smedley's own written work, interviews, and the intelligence files of a variety of government agencies that spied on Smedley.

This material is presented as an analysis of Smedley's life and its relationship to her environment. How, the authors ask, did a girl growing up in poverty in the American West become an international figure devoted to the cause of Chinese peasants? How did her feminism mesh with her attraction to domineering men and her slurs on men whom she considered "effeminate"? The study also considers her relationship with the Left and with the Socialist and Communist parties. Some readers, however, may be curious as to why she did not have more contact with the Industrial Workers of the World or the Western Federation of Miners. Mother Jones, for example, was active in strikes in Colorado in 1903 and 1913 and visited Trinidad, Colorado, the year before Smedley moved there with her family. Finally, the authors ask, why did this American radical devote her life to combating oppression abroad rather than fighting it at home?

Smedley's travels present difficulties for biographers (and a map would have been helpful to readers). The authors solve this reasonably by focusing on her time in China. The use of extensive footnotes in the sections on Europe and China provides interesting information but sometimes detracts from the story at hand. It also is not necessary to romanticize the poor to regret the authors' use of the phrase "poor white trash" to describe Smedley's family (p. 1). The writing is usually clear and precise, however, and the conclusion quite eloquent.

Smedley died in 1950, under attack by anti-Communists, just as she was to return to China. She was buried with honors outside Beijing. Just as her work contributed to the causes of anti-imperialism and revolution, the MacKinnons' study is a major contribution to our understanding of twentieth-century Chinese and U.S. history and of the actions of individuals involved in struggles outside their national boundaries, whether in China, or Ghana, or Spain, or Nicaragua.

EILEEN EAGAN
University of Southern Maine

RICHARD L. LAEL. *Arrogant Diplomacy: U.S. Policy toward Colombia, 1903-1922*. Wilmington, Del.: Scholarly Resources. 1987. Pp. xix, 194. \$30.00.

The author has in this work a dual purpose. The first is to analyze the impact of American intervention in the Panamanian revolution of 1903 (which strained U.S. relations with Colombia until the Thompson-Urrutia treaty was finally ratified in 1922) on U.S.-Colombian affairs, a theme too much neglected by diplomatic historians. The second and lesser task is to offer a mild rebuttal to his fellow diplomatic historians who have tried to refurbish Theodore Roosevelt's or Woodrow Wilson's reputation in dealing with Latin Americans or have failed to underscore their basic similarity of approach, in goals if not always in form.

Richard L. Lael is, perhaps unintentionally, sympathetic to what three American presidents confronted in the lingering debate over the "taking of Panama." Roosevelt did not particularly want a diplomatic confrontation with the Colombians, but he was not going to back off nor show much sympathy for the impact the "loss of Panama" had on successive Colombian governments. After all, as the persistence of the Panama issue in American politics revealed, Roosevelt had to resort to spread-eagle rhetoric to respond to Democratic criticism (and a Congressional investigation) of the Panama affair. Wilson's campaign in 1912 was laced with condemnations of American imperialism and promises of a new relationship with not only Colombia but all Latin America. On becoming president, he did not discard his anti-imperialist rhetoric but recognized strategic realities. Where Colombia was concerned, the Wilsonians wanted to resolve the festering Panama issue (and in fact quickly negotiated a treaty with Colombia) but continued to pursue American interests with such unconcealed selfishness that the Colombians (and other Latin Americans) routinely dismissed their preachments.

Lael correctly sees American priorities in Colombia as strategic and then economic in order of importance but tries to mesh these sometimes conflicting matters into a brief but persuasive volume. Rightly suspicious of any theory that suppresses contradictory facts, he nonetheless tries to provide a structure for understanding U.S.-Colombian relations and in so doing speaks to the economic and political forces that press on foreign policy.

LESTER D. LANGLEY
University of Georgia

JOHN A. THOMPSON. *Reformers and War: American Progressive Publicists and the First World War*. New

York: Cambridge University Press. 1987. Pp. xi, 300. \$32.50.

During the Progressive era a group of political writers called publicists rose to prominence and influence. They combined the role of popularizer with that of advocate. The best of them, including Herbert Croly, Walter Lippmann, and Walter Weyl, contributed significantly to American political thought. Throughout subsequent decades the liberal-reform tradition has continued to draw on their ideas. In the volume under review, John A. Thompson examines Progressivism through the writings of a selection of these publicists then studies the impact of World War I on the individuals and their beliefs.

Many other scholars have already explored the same approximate terrain, among them Charles Forcey, David W. Levy, Christopher Lasch, and Thompson himself in a 1971 article in the *Journal of American History*. Little in this book is unfamiliar. Thompson has created a lacework of brief, often telling quotations drawn from private correspondence as well as from published and unpublished writings by the twenty publicists (including several Socialists) who make up his sample. Surveying the nuances of Progressive thought, Thompson stresses what the publicists agreed on rather than their differences. World War I, he finds, diverted the writers from problems at home and divided them in ways that domestic issues had not. Yet, even through the debate over intervention, publicists on both sides of the issue continued to share many common aims, among them the more powerful governmental role in the U.S. economy that war would inevitably bring. Disillusionment ensued with Woodrow Wilson and the peace, a sense of defeat compounded by reaction at home. Their hopes smashed, the publicists followed diverse personal routes through the 1920s, but at heart most remained reformers of the kind they had been before the war.

The author cautions against generalizing about popular opinion from his limited sample of writers. Perhaps this is why the book as a whole generalizes so tentatively. Though evoked, analysis is rarely sustained. Symptomatically, the conclusion totals a scant three and one-half pages. Yet Thompson has marshaled sufficient evidence to have expanded on many of his quite provocative points. Emphasizing, for example, the publicists' unending frustration with the limited achievements of those years, he questions whether people who called themselves Progressives really "dominated 'the Progressive Era'" (p. 288). He stresses, as other scholars have done, that Progressivism had already fallen into decline several years before the United States entered World War I and sug-

gests without much clarification that this readied some of his publicists to welcome American intervention. These and other arguments have important implications that demand detailed, analytical examination. This the book does not provide.

Thompson writes lucidly, and Cambridge University Press has produced the volume well. Surprisingly, though, there is neither a complete bibliography nor an essay on sources. This lapse, like the absence of strong and sustained analysis, makes the book less useful than it should have been.

JOHN J. BROESAMLE
California State University,
Northridge

JUDITH STEIN. *The World of Marcus Garvey: Race and Class in Modern Society*. Baton Rouge: Louisiana State University Press. 1986. Pp. xii, 294. \$22.50.

Since E. David Cronon's biography in 1955, the literature on Marcus Garvey has grown enormously. Most of it has been useful and instructive, obliging historians to take that remarkable black nationalist more seriously than had been the habit. We had been too inclined to accept uncritically the views of Garvey's black contemporaries and rivals—W. E. B. Du Bois, A. Philip Randolph, and others—characterizing him as something of a charlatan or buffoon. Thanks to the work of such scholars as Robert A. Hill, in the past decade or so, a vast body of data has become available. Through that new material and fresh questions, Garvey has emerged as a major historical figure.

Many Garvey scholars have brought to their work a strong ideological charge. The discourse has been, therefore, overly heated, and the legitimate historical issues are not always clear. Of the book-length studies, however, it is clear that Judith Stein has written the most balanced and the most historically textured study of Garvey and his times. Stein has made extensive and imaginative use of a variety of archival sources, including FBI files. She has interviewed Garvey's contemporaries in the United Negro Improvement Association, as well as Amy Jacques Garvey and Randolph. The result is a thoughtful and provocative analysis of Garvey's movement and the broad political and social context of race reform and agitation from World War I through the 1920s.

The intelligent and full treatment of context makes this book as much a political history of Afro-American struggle and protest during the period as a biography of Garvey. That is a great strength, especially as it goes far toward explaining the popular response in the United States to Garvey's movement.

Stein's explanation of the movement itself is weighted more toward class than race analysis. She is, therefore, able to observe contradictions and ironies, such as dependence on a blue-collar constituency, which the movement had to exploit in order to succeed in its business enterprise, and the problems of management of the Black Star Line complicated by the need to use ships to recruit members at the expense of business interests. She does not lessen the failures but makes them more understandable.

Unlike some students of Garvey's movement, Stein refuses to see it as an earlier version of the racial politics of the 1960s. That, too, is a strength of the book and makes it the best work of history on the man and the times.

NATHAN IRVIN HUGGINS
Harvard University

RICHARD POLENBERG. *Fighting Faiths: The Abrams Case, the Supreme Court, and Free Speech*. New York: Viking. 1987. Pp. xiv, 431. \$24.95.

Richard Polenberg's study is a fascinating, well-written, and finely researched history of the social, political, and legal underpinnings of an important legal case in the establishment of civil liberties in America. Polenberg is compelling in his account of how a small group of anarchists was imprisoned and later deported for writing and distributing two leaflets protesting U.S. intervention in Bolshevik Russia in 1918. The book does much more than tell a story, however. Polenberg, with rich documentation, makes understandable both how government officials could justify imprisonment for distributing leaflets and how two dissenting Supreme Court justices saw in this case for the first time that such suppression was inconsistent with the First Amendment guarantee of free speech. Polenberg allows every participant in the case full voice, including the defendants, the immigrant and radical communities of which they were a part, their lawyer, the government agents who stalked them, the Justice Department officials who prosecuted them, and the judge who sentenced them.

The book emphasizes the courage and the audacity of Jacob Abrams, Hyman Lachowsky, Samuel Lipman, Jacob Schwartz, and Mollie Steimer. The country was in the midst of war hysteria. World War I was a period of consolidation and strength for the various government intelligence agencies. Alien radicals fell prey to massive deportation drives. Two leaflets tossed from the top of a building in New York City were considered a sufficient threat to the nation's security to warrant the arrest of five Jewish anarchists and conviction of four of them for sedition. (The

fifth, Jacob Schwartz, died in jail the night before the trial, a victim of police beatings and inadequate medical care.) Even the most liberal Justice Department officials and seven of the nine Supreme Court justices perceived no violation of the right of free speech in the case.

Polenberg is at his best as he tracks the evolution of Justice Oliver Wendell Holmes, Jr.'s, ideas on free speech. The year before the *Abrams* case, Holmes had written several majority opinions advocating a very narrow view of the First Amendment in times of war. But when the majority voted to affirm the conviction, citing Holmes's opinions, Holmes dissented. He explained that he no longer believed that the expression of opposition to foreign policy in wartime constituted a clear and present danger to national security. He introduced the notion of the free trade of ideas, "fighting faiths," competing for acceptance in the market as the underlying theory of the Constitution. Although his pragmatic framework is out of favor in present legal thinking, in his own time Holmes broadened the understanding of the meaning of the First Amendment.

The lasting value of the *Abrams* case and of the book turns on the eventual incorporation of Holmes's landmark dissent into the majority view fifty years later. Polenberg makes a convincing argument that the *Abrams* case, which epitomized the war frenzy during one of the grimmest periods in the history of civil liberties in America, actually contributed to strengthening the constitutional basis of free speech.

Polenberg skillfully blends legal and social history, supplemented with illuminating biographical sketches of the key figures involved in the case. His book will be accessible to students and informative to scholars of this critical period.

The author gives an especially vivid portrait of Steimer, the youngest of the five defendants in the *Abrams* case, and the only woman. While conducting research on her better-known anarchist comrade, Emma Goldman, in 1977, I met Steimer. Still staunchly committed to anarchism after fifty years in exile, she was as spirited and defiant in the last years of her life as she had been as a young girl on trial for her beliefs. Had she lived to read Polenberg's book, she doubtless would have chided him for failing to use it primarily as a vehicle to promote the anarchist cause. But I imagine she would have been pleased at his masterly treatment of the unwitting but vital role she and her comrades played in bringing the ideal of free political expression closer to reality.

CANDACE FALK
Emma Goldman Papers
University of California,
Berkeley

HOWARD JACOB KARGER. *The Sentinels of Order: A Study of Social Control and the Minneapolis Settlement House Movement, 1915-1950*. Lanham, Md.: University Press of America. 1987. Pp. xviii, 183. \$22.75.

In 1938 the head resident of North East Neighborhood House in Minneapolis rejoiced that "no violent anti-government movement has ever been hatched within the shadow of a Settlement—we have been sentinels of order" (p. 92). Howard Jacob Karger's brief monograph explores the reasons why the Minneapolis settlement houses sought "not to change the world but to better a small piece of it" (p. vii). Relying primarily on reports and speeches of the directors and their assistants, he finds that they acted out of altruism and a benign paternalism. Their efforts at social control, he emphasizes, were never malevolent or conspiratorial.

The opening chapters trace the rise of Minneapolis and the establishment of ten houses between 1897 and 1924. Most of the founders were Protestants influenced by the Social Gospel, but there were Jewish and Catholic centers, and one house worked with blacks. None of the settlements opposed this country's entry into World War I, and all supported the national campaign to Americanize the immigrant. By the late 1920s the settlements were stressing neighborhood building and combating the deleterious effects on youth of movies and dance halls. In the depression they expanded their youth programs and tried to assist unemployed adults. Social work professionalization invaded Minneapolis in the World War II era, causing settlements to focus on group work and psychiatric adjustment. In an interesting chapter on Phyllis Wheatley House, Karger argues that the existence of a settlement for blacks enabled the other houses to uphold racial equality while avoiding the more difficult task of integration. The author concludes that, although settlements "fought for moderate re-distribution of both resources and opportunities" (p. 142), they accepted capitalism and believed that class conflict "could be eradicated by the proper application of concern, understanding, and love" (p. 144).

The book summarizes national change and occasionally refers to well-known settlement leaders. But it does not draw comparisons with other cities of the same size, examine the response of Minneapolis settlement workers to New Deal welfare programs, mention changes in social work training and the decline of residency, look at conflict between ethnic groups, or try to assess the impact of settlement programs on participating neighbors. These and other topics would have enriched the analysis. Readers, however, will be grateful for

the generous quotations and the author's wise decision to let the settlement actors speak for themselves.

LOUISE C. WADE
University of Oregon

MARC A. WEISS. *The Rise of the Community Builders: The American Real Estate Industry and Urban Land Planning*. (Columbia History of Urban Life.) New York: Columbia University Press. 1987. Pp. xii, 228. \$30.00.

In the hyperbolic lexicon of real estate, a "community builder" is a developer who not only subdivides a substantial tract of suburban land but also builds and sells the houses on that land. By combining functions—planning, finance, construction, and sales—that had hitherto been kept separate, the "community builders" achieved economies of scale and speed without which the "mass suburbia" of post-1945 America would not have been possible. As Marc A. Weiss convincingly argues, the real origins of the "community builders" came not in the postwar heyday of suburbia but in the boom-and-bust era of the 1920s and 1930s.

Weiss brings to this history a profound knowledge of the real estate industry throughout the twentieth century. His first chapter is a model exposition of the structure of that industry in the 1920s. He then concentrates on home building in Los Angeles, a wise choice because the explosive growth and competition there forced the emergence of new forms of enterprise. Finally, he returns to the national scene with an analysis of the impact of the Federal Housing Administration (FHA).

Weiss says little about the business operations and major projects of the "community builders" operating in Los Angeles during the 1920s. Instead, he concentrates on the efforts of the large developers to influence the city-planning process. As one realtor put it, "City planning is the production end of our business" (p. xiii), and Weiss has little difficulty in showing the disproportionate influence the developers exerted on zoning boards and legislators. One major goal was to restrict the operations of so-called curbstoners, small speculators who sold lots—often without adequate services or title—to gullible buyers from their "offices" on the curbstone. In the 1920s, however, the developers were so averse to any government intervention—even intervention directly in their favor—that no significant legislation either aiding them or protecting the consumer was passed.

The situation changed abruptly in the 1930s when the federal government intervened to save

the bankrupt housing industry with legislation that did more to advance the goals of the "community builders" than anything they themselves had ever sponsored. Weiss is very effective in showing how FHA regulations favored the developer-builders over their smaller competitors. He might have tried harder to explain why this crucial piece of New Deal legislation so closely reflected Herbert Hoover's values or why the FHA was, as he correctly points out, "largely run by representatives of the real estate and banking industries" (p. 146).

More seriously, Weiss throughout the book accepts—or at least never questions—the realtors' message contained in the phrase "community builders." But were these developers with their standardized methods of planning and construction actually building communities? Or were they in fact community destroyers, bigger versions of the old curbstoners they replaced, peddling an even more monotonous version of the suburban dream? Weiss neglects some crucial cultural and political questions raised by his subject; nevertheless, his book effectively explores the economic origins of the suburban world of today.

ROBERT FISHMAN
Rutgers University,
Camden

GEORGE T. BLAKEY. *Hard Times and New Deal in Kentucky, 1929–1939*. Lexington: University Press of Kentucky. 1986. Pp. 252. \$25.00.

This book is another of several recent studies that endeavor to translate the New Deal into state terms. This may be the best of the lot. It sticks to the subject, avoids extraneous clutter, and treats the whole dramatic story with insight.

Historians have sifted through the ashes of the New Deal many times but nearly always with a national view. They have based conclusions about the effects of New Deal programs at the grass roots on fragmentary evidence. Because it is closer to the grass roots, if for no other reason, this work is welcome.

Using excellent archival and local sources, George T. Blakey traces the history of all of the standard alphabet agencies as they operated in Kentucky. The NRA, AAA, CCC, PWA, WPA, FERA, HOLC, FHA, NYA, FSA, RA, and TVA are all there. Blakey's method is to explain the program, discuss the leaders and implementation in Kentucky, analyze special problems, and assess the effects. It is not a new approach, but he does it well and bases it on comprehensive research.

If there is a problem with this book it is too much dealing in personal politics. The temptation

is great when there are colorful figures involved, and certainly Kentucky in the 1930s, with Alben Barkley, Albert (Happy) Chandler, Irvin S. Cobb, Ruby Laffoon, and Fred M. Vinson, had some notable personalities. But the reader probably learns more than is necessary of the political infighting and career building. On the other hand, the bloody labor war in Harlan County brought on by New Deal help to the United Mine Workers needs more extensive treatment.

Blakey concludes that beyond the immediate impact of the New Deal in Kentucky there were lasting effects. The land was enriched and improved by various programs. The economy was righted and set on a stable course. There were major changes in the condition of labor and permanent social changes that broadened horizons and weakened provincial barriers and stereotypes. Education was rescued from threatened disaster and resuscitated. Important parts of Kentucky's rich historical heritage were preserved.

In the process there were prices to be paid. Blakey implies that the New Deal made the individual more dependent on the federal government, which, he assumes, weakened state and local government. As an afterthought, he suggests that the "New Federalism" of the Nixon and Reagan administrations may have reversed the trend, but he speculates that after fifty years without the old responsibilities and financial burdens, the people and politicians of Kentucky may not want them back.

DAVID E. CONRAD
Southern Illinois University

NAN ELIZABETH WOODRUFF. *As Rare as Rain: Federal Relief in the Great Southern Drought of 1930-31*. Urbana: University of Illinois Press. 1985. Pp. xii, 203. \$18.95.

This brief book contributes much to our understanding of the rural South in the early years of the Great Depression. Nan Elizabeth Woodruff not only describes the drought of 1930-31 as a natural disaster but also relates it to the crisis of the plantation economy and shows that it provided the context for the beginning of debate over national relief policy.

In the summer of 1930 drought covered most of the Southeast and the lower Mississippi valley. It was most severe in Arkansas, and Woodruff concentrates on that state. It also devastated eastern Kentucky, to which she gives secondary attention.

Woodruff effectively relates the drought to the multiple disasters overtaking rural Arkansas. Destroying half the state's cotton crop of 1930, it coincided with price collapse and spreading bank

failures, all of which wrecked landlords' credit and left them unable to support tenants and sharecroppers. As those workers faced famine in the winter, one incident underscored their desperation. At England, Arkansas, in January 1931, destitute sharecroppers demanded food and were given rations by the Red Cross. This affair, though not violent, was publicized nationally as a food riot. In the first thorough discussion of this incident, Woodruff shows that it raised urgent questions about national relief policy.

One of these issues was the struggle between President Herbert Hoover and the Red Cross over national relief. Committed to voluntarism, the president sought to assign all responsibility to the Red Cross, beginning with the drought region. But the organization saw its purpose as disaster relief and rejected wider responsibilities for chronic rural poverty and economic collapse. Even so, the Red Cross began minimal aid in 1930. By 1932 this grew into the largest relief program ever undertaken by the agency. But Woodruff's examination of these operations reveals their inadequacy, their deference to planters' and coal operators' control of their work forces, and even their tolerance of some local efforts in Kentucky to deny relief and drive out the "undesirable" poor.

Congress was debating drought relief by the end of 1930. The author sees this as crucial to national policy as drought aid became "enmeshed" in a larger controversy over unemployment relief. Led by cotton-belt conservatives such as Senator Joseph T. Robinson of Arkansas, Congress passed a modest seed and feed loan bill to prop up the plantation system. But effective production credit to planters would almost have had to include food for their sharecroppers. This Hoover perceived as an opening wedge for general federal relief. He was correct about that; as insurgent Republicans and urban liberals pointed out, Congress could hardly aid drought-stricken farmers without giving similar help to all the unemployed. But Hoover stood firm against a federal dole, and, in the end, Woodruff notes, the seed and feed program included no provisions for food, operated entirely through landlords, and reached very few of the destitute.

In her conclusion Woodruff looks briefly beyond the drought to observe that New Deal crop control also operated within the plantation system and reinforced landlords' interests. Thus, in the words of one of her chapter titles, "not taking care of the whole people" was a consistent failure of the depression era (p. 158).

PAUL E. MERTZ
*University of Wisconsin,
Stevens Point*

VICKI L. RUIZ. *Cannery Women, Cannery Lives: Mexican Women, Unionization, and the California Food Processing Industry, 1930–1950*. Albuquerque: University of New Mexico Press. 1987. Pp. xviii, 194. Cloth \$22.50, paper \$10.95.

This book is a significant contribution to the fields of women's history and ethnic history. Vicki L. Ruiz demonstrates that Mexican and Mexican-American women played active, leading roles in unionizing efforts and labor strikes despite cultural traditions that encouraged passiveness. From 1939 through 1945 the United Cannery, Agricultural, Packing, and Allied Workers of America (UCAPAWA), a CIO affiliate, grew and flourished in California with Mexican and Mexican-American women occupying local positions as officers, committee heads, and shop stewards. By 1951 UCAPAWA was virtually dead in California, a victim of Red baiting and Teamster takeovers.

Focusing on this brief period, Ruiz analyzes the factors that encouraged women's successes in the labor movement: cultural change, a shortage of labor, and a democratic union philosophy. Although union activity is the focus of the author's inquiry, the book reveals as much about the cultural survival and adaptation of Mexicans in the United States as it does about the labor movement. Unionization of Mexican female packing-plant workers in California was possible because of a hybrid of Mexican and American cultures that the workers exhibited.

In her opening chapters on family life and "cannery culture," Ruiz details the ways in which Mexican families had adapted to life in the United States. American popular culture and advertising, directed toward female consumers, influenced Mexican women and their daughters as they did other women. Some Mexican women made independent decisions about personal expenditures and social activities, although Mexican-American culture remained highly patriarchal. Most Mexican and Mexican-American women who entered employment in the food-processing plants worked under the watchful eyes of male relatives who warned them against inappropriate behaviors. This blend of familial respect and independence that characterized "cannery culture" allowed women to organize and to demonstrate as members of their families rather than as Americanized individuals. The kin networks of the packing plants also made organizing easier than in settings where no special associations among workers existed.

The book presents eloquent Mexican women describing their roles in UCAPAWA, but it is not a full account of the union's rise and fall. The author downplays ethnic and gender conflict

within the organization and intentionally avoids examination of the politics of leaders and of the rank and file. Although Mexican women comprised between one-fifth and nearly one-half of the local leadership in California, they appear to have been underrepresented relative to their numerical strength in the firms that were organized. The ease with which Red baiters and the Teamsters undermined UCAPAWA when other CIO unions survived attack indicates that the organization never outgrew the fledgling stage. Although women's success through UCAPAWA was brief, Ruiz has established that their successes were real and that women were not tag alongs in a male undertaking.

JULIA KIRK BLACKWELDER
University of North Carolina,
Charlotte

STEVEN J. KEILLOR. *Hjalmar Petersen of Minnesota: The Politics of Provincial Independence*. St. Paul: Minnesota Historical Society. 1987. Pp. xii, 342. \$14.95.

Steven J. Keillor's book represents a significant contribution to the understanding of Minnesota political history, the early to mid-twentieth-century search for political alternatives to the Republican and Democratic parties, and the contributions and assimilation of North European immigrants into mainstream American political activity. Keillor's method of historical inquiry is to mix interviews and original archival sources in order to produce a skillful biography of Hjalmar Petersen as a newspaperman and insular politician of the Midwest. Just as important, perhaps more, he integrates his biographical subject into the larger mosaic of protest politics: Farmer-Laborism, Floyd B. Olson, Franklin D. Roosevelt and the New Deal, foreign policy from 1914 to 1968, Elmer Benson, Harold Stassen, Hubert Humphrey, the Democratic-Farmer-Labor party, and much more. His exposition is skillful and easy to read, and it appeals to both the general reader and the specialist.

This work could have been labeled "The Americanization of Hjalmar Petersen." Like many Scandinavian midwestern settlers—Petersen was born in Denmark in 1890 and arrived with his parents in America in 1891—Petersen spent most of his life searching for party loyalties and answers to the pressing issues of the time. At various times he was a Republican, Farmer-Laborite, reconverted Republican, Democratic-Farmer-Laborite, anti-Communist, isolationist, liberal, populist. His search for solutions to universal issues such as war and peace, taxation, liberty, distribution of income,

urbanization, economic development, and so on caused him to wander about a great deal over the political landscape.

The theme around which Keillor weaves this story is provincialism, by which he means that Petersen focused on the near-at-hand, the experiences of community rather than nation or world. Petersen, it is said, attempted to apply the lessons and values of the local community to conflicts of people and classes, nations, and national and world economies. Others leaped beyond local concerns: the Cowles family (the *Minneapolis Star and Tribune*), the Ridders (*St. Paul Pioneer Press-Dispatch* and Knight-Ridder), Stassen (the United Nations), Humphrey, Walter Mondale. They were the cosmopolitan internationalists, and they came much closer to expressing American ideological orthodoxy by 1938–39 and after.

The author is less quick to criticize the provincials' frame of reference. The isolationist reluctance to join the European conflict of 1914–18 may have been folk wisdom in Minnesota, but the consequences of a dictated Versailles Treaty that seems to have given rise to European fascism, World War II, and then the cold war serve as bitter reminders that sometimes the folk preference of "Don't just do something, stand there" may be the preferred policy after all.

If there is a criticism of Keillor, it is that the dichotomy of provincialism versus cosmopolitanism (or internationalism) may attempt to explain both too much and too little, even though it makes for fascinating history and biography. The provincials—call them midwestern isolationists or insularists—can be explained as having different self-interests than, say, New Yorkers and coastal-plain workers or bankers. Midwest Germans and Scandinavians fled the Continent in part to escape its conflicts, not to be drawn back into the vortex of power struggles and massive killing. To Keillor's provincial explanation should be added Minnesota's and the upper Midwest's moralistic political culture, a belief in the idea of a commonwealth (as Daniel J. Elazar has argued). Furthermore, many Scandinavian evangelicals reflected the values of the "low church" and distrusted centralized authority of any kind, whether it was found in the seminary or on the eastern seaboard, whether ecclesiastical or secular, Marxist or capitalist. None of this detracts greatly from an otherwise balanced and thorough study of a midwestern political actor of more than provincial significance.

MILLARD L. GIESKE
*University of Minnesota,
Morris*

VIRGINIA VAN DER VEER HAMILTON. *Lister Hill: Statesman from the South*. (Fred W. Morrison Series

in Southern Studies.) Chapel Hill: University of North Carolina Press. 1987. Pp. xiii, 375. \$24.95.

Lister Hill wanted to be a U.S. senator. One obstacle stood in his way: Hugo Black held the coveted seat. When President Franklin D. Roosevelt nominated Black to the Supreme Court in 1937, Hill was in Europe. Friends in Montgomery jokingly reported that Alabama's second district congressman had drowned: "Lister couldn't find a boat quick enough, so he started swimming back!" (p. 76). In truth Hill hastened home to win Black's senate seat.

When he retired in 1968, Hill received national acclaim for a distinguished career. Even his political enemies admitted the magnitude of his achievements, especially in medical care and research and education. Although the American Medical Association labeled his programs "creeping socialism," his office walls displayed plaques and awards from many prominent medical societies. Congress named the new library annex at the National Institutes of Health the Lister Hill Center for Biomedical Communications. In 1968 the Department of Health, Education, and Welfare awarded him its distinguished service award, and in 1969 he received the Lasker Foundation award for contributions to medical research. On any list of this century's ten most productive senators, Hill's name would appear near the top.

Virginia Van der Veer Hamilton writes sympathetically, yet critically, of a man she knew personally. Her political biography tells a good story about an important figure in twentieth-century politics and asks some hard questions about the nature of statecraft. Through Hill's life Hamilton explores the intricacies and ironies of southern politics.

Her account of Hill's family shows the complexities of southern ancestry. Born to privileged status in Montgomery, Alabama, Hill was the son of a Catholic mother with Jewish ancestry and an agnostic father whose forebears were Methodist preachers and planters. He was baptized and reared a Catholic. Political pressures, especially the scrutiny of the Ku Klux Klan, forced Hill to abandon his mother's faith. This would not be the last time for choosing expediency over principle.

Hamilton's biography offers a fresh look at southern liberalism by focusing on a politician rather than an intellectual. Hill was a liberal and Roosevelt's chief spokesman in Alabama. By opposing his state's "Plantation Whigs," he sought to improve the lives of ordinary folk through New Deal programs.

At the heart of Hamilton's study is the southern liberal's dilemma over race. When national liber-

alism shifted its emphasis from class to race, how were southerners to support liberal programs without disturbing the racial status quo? Hill again surrendered to expediency. He voted against every major civil rights bill that came before Congress. Remaining in the Senate to serve Alabama's needs required adherence to the South's traditional racial stance. As other southern representatives, unable to accommodate themselves to race politics, succumbed, Hill managed reelections on the strength of his admirable record. But even he ran scared.

As she recounts Hill's increasing political difficulties, Hamilton ably discusses the intricacies of state politics. During Hill's tenure in the Senate, Alabama politics changed, slowly at first, more rapidly after World War II. There had always been bitter struggle to control the state Democratic party. Race compounded bitterness, causing defections and, eventually, the growth of a two-party system. These circumstances, as much as age, finally forced Hill's retirement.

Hamilton's biography reads well. One wishes for more detail regarding Hill's private life and the relationship between it and the public man. This omission is a minor detraction. Hamilton deserves praise for a fine biography, one that gives a keener sense of southern statecraft, broader understanding of southern liberalism, finer appreciation of state and local politics, and a wistfulness over time and energies lost in playing the game of race politics.

SARAH NEWMAN SHOUSE
Alabama A&M University

PAOLO E. COLETTA. *Patrick N. L. Bellinger and U.S. Naval Aviation*. Lanham, Md.: University Press of America. 1987. Pp. x, 467. \$28.75.

Patrick N. L. Bellinger was a man of action: a pioneer test pilot who set altitude and distance records for seaplanes, the second man to be catapulted, and one of three commanders of the American planes that first attempted transatlantic flight in 1919. He became a salesman of naval air power to surface admirals reluctant to recognize its value. He helped establish two major naval air stations and commanded three aviation ships. At Admiral Husband E. Kimmel's right hand at Pearl Harbor on December 7, 1941, he trained pilots and planned for the Battle of Midway six months later. Bellinger then became deputy to Fleet Admiral Ernest W. King and oversaw the conduct of aerial antisubmarine operations in the Atlantic. Promoted to vice admiral, he served for two years on the navy's General Board before retiring in 1947. His was an Avis career—one that took him near, but never to, the top of the American naval hierarchy.

What should the biographer or historian do with this kind of figure? Paolo E. Coletta correctly argues that he must be treated as an exemplar, a man whose career is valuable less for its particulars than for what it reveals about the development of naval aviation. Yet this book is an exhaustive account of one man's experiences from 1907 to 1947. Coletta uses Bellinger's unpublished autobiography, correspondence with his associates, Italian-language materials, and a plethora of published and archival materials on the development of American naval aviation to etch a very clear portrait of the admiral. Seventeen chapters treat each step of his career, provide insights into his family life, and offer humorous incidents and samples of his vigorous language that help bring the admiral back to life. Nothing appears to be missing from the picture.

Yet one comes away from it feeling that a great deal is missing. Coletta all but drowns Bellinger in detail, leaving the storms and calms of larger events in his lifetime fogged in the background. He never explains why the admiral-to-be sought out the job of naval attaché in Benito Mussolini's Rome in 1928; nothing is said about the Italian response to the London Naval Conference of 1930; nor does Coletta explore the ramifications of Bellinger's proposal to decorate Italo Balbo, naval aviator and key aide to Mussolini. He does little to clarify whether Bellinger's tightly drawn personality and marriage to the daughter of a wealthy midwestern aviation industry financier were unique or typical of his peers in the navy.

Most important, by portraying Bellinger as a loyal officer and dutiful hero, Coletta sidesteps questions about his career that might provide new insights into the broader evolution of the U.S. Navy. What does Bellinger's graduating thirteenth from the bottom of his Naval Academy class and ending up a vice admiral say about the values and power structure of the early twentieth-century navy? What does his passage from young test pilot to middle-aged base commander suggest about changing roles and responsibilities of the naval officer? And did the emergence of technologically defined "communities" within the sea service of which Bellinger was a part serve the interests of navy and nation? By not addressing questions such as these, Coletta leaves unclear the larger significance of the admiral's career.

Thus, this portrait, despite its detail and scholarship, is one that only specialists in the history of naval aviation are likely to view. Drawn narrowly, it is unlikely to enlighten those interested in the broader history of navies or to enkindle the enthusiasm of the general reader.

ROGER DINGMAN
University of Southern California

D. CLAYTON JAMES. *A Time for Giants: Politics of the American High Command in World War II*. Assisted by ANNE SHARP WELLS. New York: Franklin Watts. 1987. Pp. xvi, 317. \$19.95.

The public demand for books about World War II apparently remains insatiable. This is the second major popular account of American military leadership during that conflict to be published in 1987. The first, by Eric Larrabee, examined Franklin D. Roosevelt and nine of his military commanders in approximately six hundred fifty pages of text. In the present book, D. Clayton James examines twice as many admirals and generals in a volume less than half that size.

As author of the superb, three-volume *Years of MacArthur* (1970–85), James is eminently qualified to write such a work. With his extensive knowledge, he analyzes in a very competent manner the careers and rise to power of seven army generals (George C. Marshall, Douglas MacArthur, Dwight D. Eisenhower, Omar N. Bradley, Joseph W. Stilwell, Mark W. Clark, and George C. Patton), five admirals (William D. Leahy, Ernest J. King, Chester W. Nimitz, William F. Halsey, and Raymond A. Spruance), four airmen (Henry H. Arnold, Carl A. Spaatz, Ira C. Eaker, and George C. Kenney), and two marine commanders (Alexander A. Vandegrift and Oliver P. Smith). This balanced group includes all the members of the wartime Joint Chiefs of Staff, all the main theater commanders, and all those who were awarded five stars for their wartime efforts.

Although at times making use of unpublished documents and oral histories, James relies primarily on biographies and memoirs in what is clearly designed to be an introductory volume for the general reader. Endnotes are limited and refer primarily to James's numerous direct quotations, and the select bibliography is limited to the best or standard works on each individual and the American high command in general. The nine content chapters are arranged in rough chronological order, and each possesses the same basic structure: an overview of a specific time and theater or issue, followed by brief biographies and analyses of two commanders associated with that theater or issue at that time. By using this organization, the author is able to cover a large number of topics, events, and individuals quite comprehensively in less than three hundred pages.

The tenth chapter offers a brief set of conclusions. Even though few of the individuals studied had commanded men in battle before the war, James notes, they were clearly the best qualified and most experienced officers available. Many had been mavericks in their early years, and almost all had been penalized by the agonizingly slow pro-

motion system that dominated the armed forces before World War II. Their key common characteristic, however, appears to have been a capacity to learn and grow even after achieving high position.

All of this should serve the nonspecialist with no background quite well. Scholars, however, will find little here they do not already know and will be disappointed by the lack of depth in both the analysis and the conclusions. This is probably an inevitable result not so much of trying to appeal to a popular audience as of trying to cover so many men in so few pages. One wishes in this regard that James had used his considerable talents and knowledge to write a longer book on fewer individuals.

MARK A. STOLER
University of Vermont

MASAYO UMEZAWA DUUS. *Unlikely Liberators: The Men of the 100th and 442nd*. Translated by PETER DUUS. Honolulu: University of Hawaii Press. 1987. Pp. xi, 259. \$19.95.

In October 1944, amid bitter fighting around the village of Bruyères in the Vosges mountains of northeastern France, a "lost battalion" of 200 Texans was rescued by Nikkei (Japanese-American) soldiers of the U.S. Army's 100th Infantry Battalion from Hawaii and the 442d Regimental Combat Team (RCT) that later developed around it with the motto, "Go for Broke." The 100th and 442d had earlier received positive press coverage for their successes in Italy, at Anzio, Salerno, and Cassino, and by war's end the racially segregated 442d RCT was lauded as "the most decorated unit in the history of the United States" (p. 231). Of an estimated 36,000 eligible Nikkei males, over 20,000 eventually served—the highest proportion of any U.S. ethnic group in military service during World War II.

This book evolved from a series of eight articles serialized in the Japanese magazine *Bungei Shunju* in 1982. Author Masayo Umezawa Duus, whose historian husband Peter Duus edited and translated this volume, treats the 100th and 442d as mechanisms to tell a larger story, describing the conditions in which the unit was formed, trained, and deployed as reflections of pervasive themes characterizing America's ambivalent wartime treatment of the Nikkei. Thus, she traces the hostile prewar West Coast attitude toward Japanese immigrants, which, two months after imperial Japan's attack at Pearl Harbor, led to the abrupt evacuation and wartime incarceration, without due process or evidence of disloyalty, of over 110,000 Nikkei in several systems of concentration camps.

The strongest feature of this volume is its emphasis on telling the story by letting the insiders speak for themselves. Reflecting recently declassified archival sources and nearly three hundred interviews with Nikkei veterans and family members, this book offers neither the conventional focus on officers (who were largely Caucasian) nor a beatification of all Nikkei who served but a wide range of candid and intensely personal accounts of why and how they went to war. The insiders' stories reveal clashes and competition caused by the often petty but persistent differences between Nikkei from Hawaii ("Buddaheads") and those from the mainland ("Katonks"). They also confirm that Nikkei saw themselves as fighting several wars simultaneously, against not only official foreign enemies but also racism and hysteria that kept their families behind barbed wire at home, politicians who saw them as convenient propaganda tools against the Axis, and commanders who seemed to view them as expendable in combat. Indeed, five of seven presidential unit citations were awarded for the twenty days of sustained combat in the Vosges forest, at the end of which the regiment was reduced to less than one-third of its authorized strength.

These qualities notwithstanding, this book complements, but does not replace, two earlier works: the numerous maps and photographs in Orville C. Shirey's *Americans, The Story of the 442nd Combat Team* (1946) and Thomas D. Murphy's copiously documented study of the 100th Battalion, *Ambassadors in Arms* (1955). Nearly one-half century later, Duus echoes both Shirey and Murphy in assessing the influence of the 100th and 442d in changing the public image of Nikkei in postwar America: "There can be no doubt that the postwar political and economic achievements of the Japanese Americans owe much to the commitment of the young men who wanted so desperately to fight for a country that had treated them at first like aliens" (p. 236).

DONALD TERUO HATA, JR.
California State University,
Dominguez Hills

MICHAEL J. HOGAN. *The Marshall Plan: America, Britain, and the Reconstruction of Western Europe, 1947–1952*. (Economic History and Policy: The United States in the Twentieth Century.) New York: Cambridge University Press. 1987. Pp. xiv, 482. \$34.50.

This is the richest, most detailed, and most complex study of the Marshall Plan that we have or are likely to get for some time. Michael J. Hogan's attention is on the United States and conflicts

within the American government. Great Britain, nonetheless, is not far from center stage, and the other nations of Western Europe are successfully integrated into the narrative.

What is best about the book is that in a sustained way Hogan illustrates what is to be gained by examining American diplomacy in the framework of "corporatism." Related to the Open Door thesis, the corporatist model sees the Marshall Plan as an extension of developments in the United States stretching back to the Progressive period. In resolving their own disputes, American statesmen aimed to create in Western Europe the sort of "neoliberal" political economy that had firmly taken root in the United States during the New Deal. In the late 1940s the Americans wanted to block the Soviet Union. Dynamically operating to accomplish goals that were independent of a fear of Russia, however, they also wanted to remake the Old World in the image of the New.

The Americans desired to build in Europe all the economic structures that they associated with a multilateral trading order. With the promise of dollars as an obvious source of power, the United States tried to bring about what it conceived as the depoliticization of Western Europe. The technocratic formulation of problems and the use of American marketing and engineering methods would lead to shared abundance and replace ideological strife both within European countries and between them. Carrying out this program would make economic growth coterminous with European social peace. Yet, as Hogan shows, the actuality of the Marshall Plan was the meeting of this set of ideals with the realities of European politics. The United Kingdom in particular shaped American wishes to its own needs. Reflecting their interest in a "special relationship" with the United States, the British desired a North Atlantic rather than a European system. This sort of obstructionism on the part of various national elites led to a different kind of Europe than that originally contemplated by New World diplomats—one in which France took the lead for European integration and Britain was not included.

The persuasive aspects of Hogan's account demonstrate that Open Door revisionism as applied to the postwar period has much life to it. The arguments of the book are also a powerful counter to the so-called postrevisionist synthesis. Without the stridency of the revisionist diplomatic history of the 1960s and 1970s, Hogan shows that the bland anti-Soviet style of postrevisionists is no substitute for critical analysis.

The failure of *The Marshall Plan* is that Hogan assiduously and almost consciously stays away from any attempt to look at the connection between the policy initiatives of the American geo-

politicians and the wider culture in which they acted. In a large and complicated text, the author is sometimes repetitious and occasionally becomes tangled up in recounting minutiae. Hogan is enthralled by the details of diplomatic maneuvering but barely mentions McCarthyism. There is no discussion of the rise of American anti-Communism, the Truman administration's public information program, or the narrowing of its options. Hogan has no interest in tying these sorts of factors to his understanding of the working out of the corporatist vision or its implementation in the late 1940s and early 1950s.

In this respect the book is retrogressive: it refuses to draw out the relation between American political culture and high policy. The author does not take advantage of some of the genuine strengths of the Open Door thesis—the elaboration of the ways in which the values of a changing political economy have generated a way of life that unites domestic and foreign affairs. But *The Marshall Plan's* vices should not cause readers to overlook its virtues.

BRUCE KUKLICK
University of Pennsylvania

RICHARD T. SYLVES. *The Nuclear Oracles: A Political History of the General Advisory Committee of the Atomic Energy Commission, 1947–1977*. Foreword by ANTHONY A. TOMEI. Ames: Iowa State University Press. 1987. Pp. xviii, 319. \$27.50.

During its existence from 1947 through 1977, the General Advisory Committee (GAC) of the Atomic Energy Commission (AEC), and, for its last three years, of the Energy Research and Development Administration, played an important role in major debates over atomic energy policies. Its members, appointed by the president, ranked among the best-known and most able scientists of their day. The activities and the influence of the GAC are worthy of careful scholarly examination; they offer a way to gain a fuller understanding of the programs and decisions of the agencies the committee advised. Unfortunately, Richard T. Sylves has only partially tapped the rich potential that his subject presents.

The greatest strength of Sylves's book is that it provides a great deal of factual information about the GAC. Anyone who wants to know who the members of the committee were, when they served on the GAC, what their scientific qualifications and major research projects were, or where they went to school will find the book a valuable reference. So will those who are interested in the functioning of the GAC. Sylves describes not only the key issues it discussed but also the appoint-

ment of its members, the role it played in awarding scientific prizes, and other administrative matters.

The greatest weakness of Sylves's book is that he fails to explain the meaning or importance of the information he provides. He devotes considerable space, for example, to showing that a large number of GAC members were graduates of the Massachusetts Institute of Technology or the University of California at Berkeley and to pointing out that many of them were veterans of the Manhattan Project. But he does not attempt to analyze the significance of those facts for understanding the status, policy positions, or recommendations of the GAC. In a chapter on the nuclear testing controversy of the 1950s, Sylves notes that at a meeting in 1958 the committee unanimously expressed grave concern about the effects of radioactive fallout. Yet a year later it issued a public statement that echoed the AEC's assurances that fallout was not a major public health hazard. Sylves does not try to account for this dramatic shift or to evaluate the personal motivations or institutional pressures that might have prompted it.

On some occasions Sylves nettles the increasingly frustrated reader by raising important questions but declining to answer them. He suggests, for example, that an effective advisory committee will have "a measurable impact" on its parent agency (p. 251). But he makes no systematic effort to measure the influence of the GAC on the AEC. On other occasions Sylves seems to contradict himself. He asserts that the prestige of its members shielded the GAC from undue pressure from the AEC. In the next paragraph, however, he submits that the AEC exploited the stature of the GAC to advance its position during the fallout controversy.

In short this is a useful but disappointing book. The subject promised much more of interest and importance than the author delivers.

J. SAMUEL WALKER
U.S. Nuclear Regulatory Commission
Washington, D.C.

DAVID F. NOBLE. *Forces of Production: A Social History of Industrial Automation*. Paperback edition. New York: Oxford University Press. 1986. Pp. xviii, 409. \$8.95.

The latest wrinkle (an old-line machinist might call it a "shop-kink") in the history of technology is a spillover into the field by sociologists of science. With their talk of networks, systems, and "integrated social constructivist approach," some have recently sought to establish a "Social Construction

of Technology (SCOT) programme." Without the help of such social science models, David F. Noble expertly demonstrates that, as the political theorist Langdon Winner claimed some time ago, artifacts do indeed have politics.

Noble's purpose is to show how the industries of aircraft production, electronics, and machine-tool manufacture, drawing on the scientific and engineering talent of the Massachusetts Institute of Technology and spurred on by the antilabor policies of such corporate giants as General Electric and by the cold-war agenda and technological vanguardism of the Air Force, pioneered the field of industrial automation. The project, not surprisingly, was lavishly funded by public monies. As Noble clearly shows, technical limits, profit maximization, and the discipline of the market, all presumed shapers of our technology, were all subverted and overridden by military fantasies of perfection and corporate fantasies of total control of the workplace.

The resulting innovation of numerical-control (N/C) machine tools, designed to eliminate the skilled machinist and perform miracles of precision, perfectly fit the social and political agendas of those who paid for and controlled the process. The rival record-playback (R/P) system, which would have done neither but would have better suited the needs of most civilian companies, died for lack of patronage.

By 1945 World War II had generated a virtual "electronic environment" for warfare, many of the components of which could presumably be taken up by civilian industry. The computer was a prime example, but less dramatic servo-mechanisms were also of potential value. In the context of a cold war abroad and a war against labor's gains of the 1930s and 1940s at home, inventors, engineers, and the Air Force dreamed of automating the machine shop. In 1946 *Fortune* magazine wrote of the "Automatic Factory" and the "threat and promise of laborless machines." Almost alone among the experts, MIT's Norbert Wiener refused to cooperate. In 1949 he rejected the offer to consult with industry and instead warned Walter Reuther of the United Auto Workers of the approaching danger of automation. It was a lone and ineffective voice. As Noble ably demonstrates, the supposed categories of economic and technical viability were in fact neither economic nor technical but in essence political and cultural.

Noble's research is exhaustive, his technical descriptions are full and understandable, and his writing is lively. The untimely death of R/P and the unlikely triumph of N/C tools are not exceptional. As he says, "this is not a book about American technology but about American society" (p. xi). By demystifying the former, he allows us

better to see and understand the way the latter really works. This book will be a classic in the field.

CARROLL PURSELL
Case Western Reserve University

BURTON I. KAUFMAN. *The Korean War: Challenges in Crisis, Credibility, and Command*. Philadelphia: Temple University Press. 1986. Pp. xi, 381. \$34.95.

Burton I. Kaufman has written a balanced, straightforward account of American involvement in the Korean War that will be especially useful in the classroom. This is a diplomatic history of the decisions taken by the administrations of Harry S. Truman and Dwight D. Eisenhower in regard to Korea, but Kaufman also has much to say about the military conduct of the war and the varied and complex reaction to the war in America of the early 1950s. The book is based mostly on wide reading of the recent scholarly literature, with some reference to declassified documentation to be found in the *Foreign Relations of the United States* and elsewhere.

The author offers a succinct and correct definition of this war at the outset: "a great power struggle between the United States and the Soviet Union superimposed on a civil war between North and South Korea" (p. 1). The civil aspects of the war remain murky in his account, however, as in so much of the literature. The author suggests that the North Korean decision to launch a conventional assault against the South in June 1950 was unilateral, apparently done "without the knowledge of either the Soviet Union or the PRC" (p. 32). This may well be true and illustrates Kaufman's willingness to depart from the received wisdom. But the author slights the internal fighting of 1946-50—rebellions, guerrillas in the interior, important clashes along the parallel—which constitutes the essential backdrop to P'yongyang's strategy in June 1950 and the experience defining the civil and revolutionary nature of the North-South conflict.

Kaufman has a nice grasp of "the great power struggle," however, and a good eye for new information that requires revision of conventional judgments on the American role. He understands, for example, that the American march to the Yalu reflected a critical shift from containment to rollback—in Washington, not just in Tokyo (p. 60). He provides one of the best accounts in the literature of the depths of the crisis detonated in Washington by Chinese intervention in Korea, a crisis worse than Cuba in 1962 (in my view) and inexplicable without reference to the coalition for rollback inside the Truman administration.

Kaufman is also very good at presenting the

range of American domestic opinion in winter 1950–51, from *Commonweal*'s dark prospect recalling "the days of Augustine and 'The City of God,'" as Rome fell "before the onslaught of the barbarians," to Herbert Hoover's eloquent neo-isolationist call for a "Gibraltar of Civilization" in a self-sufficient Western Hemisphere (pp. 121–23). But, as Kaufman shows, the Chinese mainly wanted to teach the lesson that rollback could not work, thus having the effect of returning the administration to its original concern in June 1950: containment (p. 138).

The rest of the account offers a fair appraisal of Douglas MacArthur's removal, a detailed look at the "peace process" that finally achieved an armistice in 1953 about where it might have been reached in 1951, and the initiatives of the Eisenhower administration that finally brought America's first "peace without victory." Kaufman's conclusions to this good book are judicious, particularly in his critique of Harry Summers's revisionist view that the United States could have "won" in Vietnam by treating that war as another Korea—as a conventional war, not a guerrilla war. Yet the truth, still so little understood, is that both the Korean and Vietnam wars mingled conventional and unconventional warfare, because both were civil and revolutionary wars in their essence, overshadowed by great-power involvement.

BRUCE CUMINGS
University of Chicago

YVONNE YAZBECK HADDAD and ADAIR T. LUMMIS.
Islamic Values in the United States: A Comparative Study. New York: Oxford University Press. 1987.
Pp. viii, 196. Cloth \$24.95, paper \$9.95.

This is a pioneering study on a subject about which very little is known. Yvonne Yazbeck Haddad and Adair T. Lummis discuss the Islamic values that American immigrants honor and how these values are interpreted and practiced in a new social environment. Since most of the early immigrants from Islamic lands were Christians, Islam as a religion was not the cause of tension, although some of its social values, which Levantine Christians share, may have created some problems. In recent years, especially since the 1950s, following revolutionary upheavals, regional wars, and worsening economic conditions (in particular in South Asia and Africa), the number of Muslim immigrants has been steadily increasing. Tension and fear of compromising the faith and values have become the subject of wide concern among Muslim immigrants as a by-product of the processes of assimilation and accommodation to the American social milieu. This study, however, does not deal

with the social background and the conditions that prompted both rich and poor Muslims to emigrate to the United States; it addresses itself to the impact of the American way of life on the Islamic values that are now undergoing thorough scrutiny by Muslim immigrants.

This is not the first time that Islamic values have been exposed to foreign influences, but it is perhaps the first time that Muslim immigrants have seriously questioned the suitability, if not the validity, of their values to the new country they chose to live in. In earlier centuries, when Islamdom was on the ascendancy, Muslim immigrants entered into dialogue with Christians and with full self-confidence were reinterpreting and developing Islamic values. This tolerant attitude encouraged non-Muslims in countries that passed under Islamic rule to adopt the Islamic religion. Even after Islamic power began to decline, Islamic immigration and the spread of Islam in South Asia and Africa by trade and propaganda did not stop, partly because of the simplicity of the Islamic faith and partly because Islamic civilization was highly regarded in those areas.

Today, conditions inside and outside the Islamic world have completely changed. Muslim states have lost the power and prestige they enjoyed in the past. They are not united, despite the resurgence of Islam, in the defense of the faith. Perhaps even more important, they have lost self-confidence in their confrontation with foreigners, although they still believe in the validity of their faith and values. Needless to say, they are no longer the renowned crusaders they once were.

Against this background, it is not difficult to understand the predicament of Muslim immigrants in their grappling with the problems of security, discrimination, and fear. This situation, however, is not the same throughout the country; it varies from one community to another. By interviews with individuals in selected areas the authors of the present study have tried to examine the experiences of each Muslim community in dealing with its problems. They chose three areas: the Midwest, upper New York, and the East Coast. In each they selected individuals and institutions who were sympathetic with their project and who were prepared to cooperate with them. I have no quarrel with this approach, for the data are examined in relation to their cultural background. My only reservation is its application largely to the poorer communities, although occasional references are made to the intellectual and the relatively rich communities.

Today no organized Muslim immigrants are engaged in proselytizing or could hope to win converts even if they tried, although a number of Americans have been converted. But Muslim im-

migrants do expect Americans to respect their faith and essential values. As the data of the present study indicate, frustration and tensions among most Muslim immigrants have been generated mainly by discrimination. Fear, real or imaginary, from a hostile social environment seems to have aggravated tensions and prompted many Muslim immigrants to take a defensive attitude toward Islam even though they realize that some of its values are no longer relevant to modern conditions.

MAJID KHADDURI

*School of Advanced International Studies
Johns Hopkins University*

HARRY J. AUSMUS. *Will Herberg: From Right to Right*. (Studies in Religion.) Foreword by MARTIN E. MARTY. Chapel Hill: University of North Carolina Press. 1987. Pp. xviii, 275. \$29.95.

Harry J. Ausmus has written neither a biography of Will Herberg nor an intellectual history of his ideas but rather a chronicle of his publications capped by a brief critical essay. The essay is sharp and insightful, but it bears little relation to the lengthy and repetitive paraphrases that precede it. Ausmus's insistence on the continuities in Herberg's shift from Lovestonite "right-wing" Communism to a Niebuhrian "right-wing" Judaism would have been better served by a more consistently analytical approach throughout his book. Instead, he has chosen to imitate Herberg's pedagogical method, making a rigid distinction between summary and critique that is ultimately unsatisfying as history.

Ausmus views Herberg's life as a case study of the theory advanced in his previous book, *The Polite Escape: On the Myth of Secularization* (1982): that modern politics consists of a "disguised theology" (p. xv). As Marxist and Niebuhrian, Herberg searched for an eschatology of redemptive history and a transcendent perspective from which to criticize contemporary American culture. In *Judaism and Modern Man* (1951) and *Protestant, Catholic, Jew* (1955), that search led to the endorsement of prophetic Judaism as an existential creed that challenged the "idolatrous" complacency of modern life. But, as Ausmus recognizes, Herberg never confronted the "covert nihilism" within the prophetic tradition, which must acknowledge that even the prophets speak from a human perspective that is, by their judgment, meaningless (p. 220). Herberg's refusal to admit the contingent nature of prophetic knowledge made his own claims for a "transcendent" faith as idolatrous and idealistic as the secular liberalism he despised. Ausmus's penetrating critique of Herberg's pro-

phetic stance makes his concluding chapter an important contribution to the current discussion of American politics and religion that deserves to be read alongside Herberg's two major works.

The same cannot be said, unfortunately, of the rest of Ausmus's monograph. His attempt to capture Herberg's thinking at every stage of his career makes for dull reading. Despite the detail, Ausmus never fully accounts for changes in emphasis in Herberg's thought, nor does he evaluate Herberg's relation to the contemporaries who influenced him. The absence of any examination of Reinhold Niebuhr's work is particularly a problem, since so much of Herberg's mature work was indebted to Niebuhr—to the point where Herberg's literary style mimicked Niebuhr's rhetoric. Attention to Niebuhr's career would shed light on Herberg's conversion to Whittaker Chambers's conservatism, which Ausmus does little to explain. Why did Herberg interpret Niebuhr as a Burkean conservative, rejecting Niebuhr's belief that his theology strengthened the resolve of those committed to social justice? Unlike Richard W. Fox's *Reinhold Niebuhr* (1985), Ausmus's book neglects the broad Niebuhrian context of both Herberg's conservatism and Martin Luther King, Jr.'s, radicalism. Herberg's hysterical attacks on King in the 1960s suggest the need for closer attention to the postwar discourse of prophetic "realism," a subject that is largely missing from Ausmus's work.

CASEY BLAKE

Indiana University

WAYNE S. SMITH. *The Closest of Enemies: A Personal and Diplomatic Account of U.S.-Cuban Relations since 1957*. New York: W. W. Norton. 1987. Pp. 308. \$19.95.

In July 1979 Wayne S. Smith went to Cuba as chief of the United States interests section—a post that carried with it the salary and perquisites of an ambassador. He came to the job well prepared. A no-nonsense Marine Corps veteran, Smith studied at Mexico City College before joining the State Department in 1957 as a junior research analyst. In his first assignment he looked into allegations that Fidel Castro, then fighting in the Oriente mountains, was a Communist. The team concluded, after an exhaustive investigation, that the available evidence could not substantiate that view. Smith continued his work on Cuba and a year later was sent to that country as a visa officer, later transferring to the political section. He stayed through the most critical period of the Castro revolution. He found the embassy atmosphere under Ambassador Earl E. T. Smith "poisonous." In contrast, he writes, Philip Bonsal was a knowl-

edgeable professional diplomat, "patient and forbearing" in the face of Castro's provocations. To Smith, John Foster Dulles seemed more interested in cold-war politics than in social reform in Cuba. If he comes down harder on Republican administrations, chiefly Dwight D. Eisenhower's and Ronald Reagan's, he does not hide his feelings about John F. Kennedy and Jimmy Carter. He characterizes American policy toward the Cuban revolution, from the Batista years to the present, as a succession of failures to take advantage of opportunities—to support moderates such as Ramón Barquín, for example, or to push in 1959 for economic aid to the government of Manuel Urrutia. But he sees, correctly, that Castro's decision to embrace Marxism-Leninism and to propel Cuba into the orbit of the Soviet Union was essentially a matter of foreign-policy objectives, not of his domestic program. It was no accident, writes Smith, that Castro's revelation that Cuba was a socialist country came on the eve of the April 1961 invasion. It was an urgent call, Smith says, for Soviet aid when the Cuban prime minister was convinced that an American invasion was imminent.

When the outgoing Eisenhower administration severed relations with Cuba in January 1961, Smith returned to Washington to write daily "Cuban situation reports." After the Bay of Pigs fiasco he served two years in Recife, Brazil. Back in the United States in 1963, he enrolled in the Latin American Studies program at Columbia University and then spent two years in the Cuban Affairs section of the State Department. In 1966 he shifted to the Soviet section, where his chief responsibility was the analyzing of Cuban-Russian relations. After a two-year stint in Moscow, he was posted again to Latin America, serving in Buenos Aires from 1972 to 1977. The Carter administration, eager to open up new avenues to the Castro government, summoned Smith to Washington to work again on Cuban affairs. He played an important role in the negotiations that led to the temporary rapprochement between the two countries. In 1981, however, he found himself increasingly at odds with Reagan's Latin American policy. The department ignored him, refused to reply to his dispatches, and bypassed him in approaches to the Cuban government. Smith might have accepted an ambassadorship in a small African state—Washington's ploy to get him out of the way—but he preferred to ask for early retirement. Subsequently, he wrote editorial-page critiques of Reagan's "failures" in Latin America, and his dissenting voice could be heard frequently on national radio and television networks.

Ostensibly a diplomatic history of U.S.-Cuban relations during the Castro years, this book is based chiefly on Smith's firsthand experiences. To

provide a running narrative, he cites a few standard and fairly reliable English-language sources. Sometimes, however, the bones show through, as when he relies too heavily on John Stockton's partisan version of events in Angola. Smith is at his best in the last chapters of the book, by far the largest section, when he deals with the Nicaraguan revolution, the crisis of the Peruvian embassy, the Mariel exodus, the Grenada invasion, and, especially, his own worsening relations with a politicized State Department. Smith wears his heart on his sleeve. He loves Cuba and the Cuban people. Yet he is not "soft" on Castro's Communism. His account of a government-organized attack outside the embassy building on defenseless Cuban civilians hoping to emigrate is a chilling testimony to the brutality of an authoritarian regime. But Smith believed that it was better to discuss differences, if possible, than to fight over them. As he was preparing to take leave of his post, and of Cuba, he reported to the department proposals by Raúl Castro for negotiations on Central America. He received no answer. Instead, Thomas Enders, the assistant secretary of state for American republics affairs, told Congress that the Cubans had refused to negotiate. Smith writes: "I was deeply ashamed of my government" (p. 267).

ROBERT E. QUIRK
Indiana University

BEAT BUMBACHER. *Die USA und Nasser: Amerikanische Ägypten-Politik der Kennedy- und Johnson-Administration 1961–1967*. (Beiträge zur Kolonial- und Überseegeschichte, number 38.) Stuttgart: Steiner, 1987. Pp. 308. DM 64.

Beat Bumbacher's study analyzes the policies of the John F. Kennedy and Lyndon B. Johnson administrations toward Gamal Abdel Nasser. Bumbacher describes how the Dwight D. Eisenhower administration managed a relationship with Nasser and Egypt that deteriorated rapidly after 1954, reaching a confrontational stage in 1958, before cooling down somewhat in the waning years of the Eisenhower presidency.

Despite the tension in U.S.-Egyptian relations, Bumbacher views the Kennedy administration as optimistic about its Egyptian policy because the relationship could hardly become worse and because Kennedy admired Nasser's political skill. Kennedy initiated a policy of "selective cooperation." Nasser's willingness to place the Arab-Israel question on hold allowed Kennedy political space to move closer to Israel. By 1963 U.S.-Egyptian relations had become tense again for various reasons. Most telling was Nasser's decision to interject Egyptian power into the Yemen inter-

nal disturbance and thus to anger the Saudi Arabians. The turning point in deteriorating U.S.-Egyptian relations, in Bumbacher's view, was the failure of U.S. mediation efforts in Yemen and the consequent firm decision of the U.S. Congress to cut off agricultural aid programs to Egypt. In 1965 Nasser indicated a willingness to reenter "selective cooperation." He obtained a wheat deal to alleviate a persistent food shortage but little else to help Egypt's sagging economy. Bumbacher argues that Nasser, faced with growing internal economic problems, sought to direct Egyptian anger outward through war with Israel in the summer of 1967.

This study relies on the Kennedy and Johnson presidential libraries as well as published works in German and English with the addition of some French-language materials. However, Bumbacher overlooks several standard studies: Raymond Baker's *Egypt's Uncertain Revolution under Nasser and Sadat* (1978), Richard H. Dekmejian's *Egypt under Nasir: A Study of Political Dynamics* (1971), and one critical Egyptian study, Anouar Abdel-Malek's *Egypt: Military Society, the Army Regime, the Left, and Social Change under Nasser* (1968).

THOMAS SCHOONOVER
University of Southwestern Louisiana

MICHAEL O'BRIEN. *Vince: A Personal Biography of Vince Lombardi*. New York: William Morrow. 1987. Pp. 457. \$19.95.

Vince Lombardi was the most successful professional football coach during the 1960s, when the sport captured the national spotlight, and was one of the finest in the history of the game. He compiled a ten-year record of 96-24-6 and established high standards of excellence and consistency. He built a dreadful Green Bay team that had previously won a single game into NFL champions in the 1961, 1962, and 1965 seasons and winners of the first two Super Bowls.

This intelligent, well-researched biography emphasizes Lombardi's development as a leader and a human being. Michael O'Brien has interviewed over two hundred people who knew him well, including former colleagues, players, friends, and family. He has examined family records, various private papers, and the files and periodicals of schools that Lombardi attended or coached at. The record is set straight on various aspects of Lombardi's life, including his mediocre academic record at Fordham, from which he had supposedly graduated with honors. Lombardi was an undersized lineman on the fabled Seven Blocks of Granite who chose coaching as his vocation and excelled at every level.

O'Brien finds the roots of Lombardi's success in his fierce ambition and egotism that was guided by a highly organized, strict, and efficient Jesuit training whose end products were expected to be dedicated, committed, self-sacrificing men who respected authority, duty, and human dignity. Lombardi was a winning coach because he was totally dedicated to his job and had the ability to discern opponents' strengths and weaknesses, recognize his own team's capabilities and flaws, and train, teach, and prepare his squad for each contest. Lombardi believed players would be most likely to reach their potential if they were mentally and physically tough and understood their responsibilities. He kept assignments simple, taught by rote repetition, and won through superior execution. Lombardi was a great motivator ("Winning is not the best thing, it's the only thing") whose players were driven to win out of fear. The coach was renowned for his angry tirades, hatred of opponents, and embarrassment of players. Yet Lombardi, who had great respect for traditional values, played by the rules, and his teams exemplified the virtues of hard work, human dignity, and meritocracy. In his personal life nothing was allowed to get in the way of his work, which made him a negligent father and a rude, inconsiderate public figure. Still, O'Brien feels he was a complete human being who could be kind, charitable, compassionate, and, in private, apologetic.

O'Brien effectively analyzes what is implicitly the professional and personal development of a leader. Lombardi exemplified a leadership style of the severe taskmaster who pushes people to their limits and thereby gains their respect, if not their love. There are presumably other models of coaching in the NFL, but O'Brien does not venture beyond Lombardi. The author goes overboard in his detailed review of Lombardi's beliefs and attitudes and his off-the-field behavior, and more attention could have been given to the NFL in the 1960s, particularly the Super Bowl with which the Packers were closely identified. Nonetheless, this is an excellent biography.

STEVEN A. RIESS
Northeastern Illinois University

JOHN R. HALL. *Gone from the Promised Land: Jonestown in American Cultural History*. New Brunswick, N.J.: Transaction. 1987. Pp. xx, 381. \$29.95.

In time we were bound to get a good book about Jim Jones and Peoples Temple. First, however, we needed to absorb the flood of reportage that treated the "mass suicide" at Jonestown as an "atrocious" forced on the "brainwashed" and socially marginal "dupes" of a psychologically de-

ranged, sexually depraved, and drug-crazed leader. As John R. Hall argues in this book, this first wave of interpretation forms an essential part of the story. The significance of Peoples Temple lies not merely in what happened to nine hundred, mostly black, Americans self-exiled in Guyana but in what happened to the rest of us who, in trying to make sense of the event, rushed to accept interpretations that made heroes of the "concerned relatives" of the Jonestown "captives" and the "martyred" Congressman Leo Ryan.

Hall has written a biography of Jones and a fascinating narrative of the activities of those in his movement. Beyond that, and of more importance, Hall asks us to understand Peoples Temple as the product of cultural norms it supposedly transgressed. The story is not quite the counterimage of what has previously been written. The deaths at Jonestown, although analyzed as an example of what Emile Durkheim called "altruistic suicide," were not redemptive either for those who perished or for those few who survived. The suicides were the final bonding ritual of people who poured out their vengeance against a world that Jones and their own experience had taught them to regard as utterly corrupt. Once it had been performed, there was no legacy, certainly no religious one. Peoples Temple, during over two decades of existence, was an oppositional group that never resolved its contradictory effort to fuse "the central dilemmas of modern Christianity: personal salvation versus the social gospel, with the philosophical antithesis of Christianity—a 'godless' yet prophetic vision of communism" (p. 303). Hall does not seriously question the view that Jones was in the end repressive and murderous.

Nonetheless, Hall's account is revisionist. He constructs his account with sentences that liken Peoples Temple to left-leaning organizations, conventional churches, Father Divine and black Pentecostal churches, Jewish and Christian martyrs, Puritan settlers, and a long list of other phenomena that previously have been put through the scholarly process of cultural normalization. The asserted likenesses cumulatively justify the conclusion that Jones was something other than a self-aggrandizing charlatan who preyed on the elderly and the poor. Peoples Temple at one point provided its members with a highly efficient, innovative network of social services. In much of what went wrong, Jones was merely mimicking "institutionalized practices in the wider society . . . the pathways of the wider world" (p. 309).

These are challenging perspectives. But perhaps the most compelling revisions in the book are Hall's harsh judgments of the motives and tactics of many who opposed Peoples Temple. One is finally led to ask what made the fate of Peoples

Temple different from that of other tightly bound religious communities that survived despite their willful courting of self-destruction. Hall suggests that Peoples Temple was a victim of a technologically shrunken world. It was not possible to escape by fleeing to Guyana. Unlike the Mormon experience with migration, it was not possible even to gain time. Jones, in his last remote outpost, remained perpetually trapped by his hatreds and the hatreds of enemies who had the power to destroy him. However difficult to imagine a happy ending for Peoples Temple, perhaps had Congressman Ryan stayed home and tended to social problems that for a long time made Peoples Temple seem progressive those who chose life in the jungles of South America, and they did choose, might have found a less terrible peace.

R. LAURENCE MOORE
Cornell University

RALPH KETCHAM. *Individualism and Public Life: A Modern Dilemma*. New York: Basil Blackwell. 1987. Pp. xiii, 241. \$24.95.

The attempt to understand and explain American civilization has a long history, dating back to Alexis de Tocqueville's *Democracy in America* and before, and includes distinguished native writers as well as foreigners. The years following World War II especially seemed to stimulate such wide-ranging examinations, as demonstrated by Daniel Boorstin's *The Genius of American Politics* (1953), Louis Hartz's *The Liberal Tradition in America* (1955), and Max Lerner's *America as a Civilization* (1957). *Habits of the Heart* (1985), by Robert Bellah and associates, represents a notable recent addition to the literature, using a sociological perspective. Ralph Ketcham's new volume may be added to the list of works that, like the Bellah book, probe the soul of contemporary America while focusing on individualism. The volume's major novelty is the attempt to place American public life in sharper perspective through comparison with the Confucian tradition of East Asia. Unlike most post-World War II analysts who celebrated American achievements, Ketcham sees much to criticize.

The book's opening chapter, "Paradoxes, 1945-85," sets the scene for what is to follow, portraying modern America as an affluent society filled with lonely people, whose excessive commitment to individual freedom has produced increasing social malaise. Recalling Hobbes's famous observation concerning life as "nasty, brutish, and short," Ketcham ponders whether, three centuries later, contemporary humanity has not achieved a condition as "solitary, rich, nasty, brutish, and long" (p. 17). The least satisfying section of the

book, this chapter presents a familiar, almost trite, litany of complaints, with little effort to update or support the charges.

Since the key to America's contemporary malaise, in Ketcham's view, is its commitment to unrestrained individualism, it is crucial that he explain individualism's historical role, an enterprise that he undertakes in his second chapter. Tracing the concept from the ancient Greeks through John Stuart Mill, the author contends that over the centuries the social dimension in Western thought has been increasingly undermined. Where the Greeks, led by Aristotle, had championed a "social individualism," balancing individual freedom and social responsibility, the Reformation, scientific revolution, and romanticism had by the nineteenth century created a philosophy of radical individualism, which now dominates American public life. An obvious rejoinder to such criticism is that one should not overlook individualism's more positive, liberating effect on the West, a point that Ketcham notes but quickly passes over.

In "East Asian Counterpoint: The Shadow of Confucius," Ketcham contrasts Western notions to the concepts of society, the individual, and the state held by the Confucian societies of China, Japan, and Korea. (The difficulty with citing the three Asian societies as Confucian is that in modern times all have repudiated the Confucian model as an obstacle to development.) Basing his analysis primarily on a small number of secondary works, Ketcham points up the contrast offered by the Confucian emphasis on social obligation, hierarchical relationships, and the subordination of individual desires to the collective good. Arguing that modern Americans sorely need some alternative to individualism, Ketcham suggests that East Asia offers a "vast reservoir of alternate values and practices" that could provide better perspectives (p. 132).

In his final chapter Ketcham hones in on the 1980s, focusing particularly on the idea of citizenship, the role of leadership, and the decision-making process in modern America. Arguing that an individualistic emphasis on the priority of self-interest and a mechanistic approach to decision making have brought the American political system near bankruptcy, he calls for a future public philosophy based on recognition that people have social natures and that disinterested action in the name of the public good is possible. Despite his harsh portrait, Ketcham ends on a moderately hopeful note, suggesting that even small changes (for example, more emphasis on the value of disinterestedness in schooling, a national initiative and referendum tied to electronic media information systems) would produce more enlightened

public decisions. Though primarily concerned with the United States, Ketcham contends that the trends he analyzes are rapidly becoming worldwide.

Such a brief synopsis cannot do justice to the sweep and complexity of Ketcham's analysis. The book is a philosophical essay rather than a work of historical scholarship, and one's judgment of it will depend more on agreement or disagreement with the argument advanced than on the supporting evidence, which is fairly slight. The book's strengths are the wide range of its coverage and the contemporary significance of the issues it raises; its weaknesses are its sketchiness, inevitable in a small book that attempts to cover so much ground, and a prose style that is often convoluted. Obviously a work of strong personal conviction, the volume may be recommended to all readers concerned about the present and future direction of American public policy.

CARL T. JACKSON
University of Texas,
El Paso

CANADA

PHILIPPE JACQUIN. *Les Indiens Blancs: Français et Indiens en Amérique du Nord (XVI^e-XVIII^e Siècle)*. Paris: Payot. 1987. Pp. 310. 160 fr.

This book is the first monograph to deal extensively with the *coureurs de bois* and, more especially, with the way these men who worked in the fur trade lived with the Indians and were assimilated to varying degrees by them. The study concentrates exclusively on French-Indian relations in the regions dominated by New France from the colony's earliest days to the beginning of the nineteenth century. The work shows considerable scholarship in the author's exhaustive archival research and his profound understanding of Indian, colonial, and European history. He presents a rigorous analysis and manages to avoid the many pitfalls associated with the stereotypes of "the good Frenchmen and the Indians living in harmony with nature."

An overall picture of native societies within this sphere of influence is presented, essentially on the basis of Algonquian and Iroquois societies and their characteristics, such as kinship societies, traditional commerce, and diplomacy. This synthesis, however, has the drawback of overlooking the distinctive features of societies in the Mississippi Valley and the Plains. The author then describes the way in which Indians and Europeans perceived each other. He goes on to deal with the insertion of Frenchmen into Indian societies, di-

viding this process into three periods. At first there were the company men, called *truchements* (or interpreters), who lived with the Indians to learn their languages and who eventually became completely assimilated. Then came the missionaries, who also lived among the Indians, with the goal of reversing the direction of this assimilation. The final phase began with the destruction of Huronia and the penetration of the *coureurs de bois* into the *Pays d'En Haut*. In this period the Indians' assimilative strength was evident to varying degrees, ranging from the transformation of these travelers into Indians and the appearance of Métis communities to the adoption of many Indian characteristics in the personal and cultural behavior of even those who returned to settle permanently in the lower St. Lawrence Valley.

Who were the *coureurs de bois*, and what attracted them to this way of life? They were soldiers, deserters, enlisted men, and volunteers, attempting to escape the hierarchy of colonial society and the difficulties of marrying there (men greatly outnumbered women); they hoped to find wealth as well as adventure. Philippe Jacquin identifies the cultural elements that helped these men adapt to Indian society: they shared similar magical beliefs, a respect for hunting, and a contempt for farm work (for one group, this was women's work, while, for the other, it was labor for serfs). Colonial authorities detested these men over whom they had no control but on whom they nonetheless depended for the beaver pelts that were the basis of the colony's economy.

The author offers no explanation of why the "white Indian" phenomenon was more prevalent in New France than in the other colonies. Nor does he deal with the question of Canadianization, even though Indian influence brought the symbolic distinction between French and Canadian into sharper focus. More important, the book leaves the reader wanting to know much more about relations between the *coureurs de bois* and the tribes that acted as intermediaries, chiefly the Ottawas and, in a later period, the Foxes. It would be also interesting to be provided with a better explanation of how the Métis communities appeared. Was there a relation between this development and the female hypergamy represented by the marriage of a fur trader with an Indian woman?

Although many questions remain unanswered, this book constitutes a major contribution to the history of French-Indian relations in North America.

DENYS DELAGE
Laval University

JANE ERRINGTON. *The Lion, the Eagle, and Upper Canada: A Developing Colonial Ideology*. Kingston, Ont.: McGill-Queen's University Press. 1987. Pp. 272. \$30.00.

Even in this enlightened age, historians still write history that is affected by the times in which they live. During the 1960s and 1970s, much historical writing in Canada was in a nationalistic vein, one of the characteristics of which was an insistence that from the time of the American revolution English Canada had diverged sharply from the United States. The loyalist settlements of the 1780s, it was argued, brought to Canada a distinctive political and social culture that was conservative and monarchical and, therefore, radically different from the democratic and republican culture of the new United States. Jane Errington has written a useful corrective to this viewpoint. After doing a prodigious job of reading through pamphlet literature, colonial newspapers, and private papers, she has shown how many links there were between the settlers in Upper Canada and the United States. "Over the years," she concludes, "strong ideological, social, and economic ties had developed between members of the colonial elites and federalists in the United States" (p. 7). She acknowledges that with this kind of research, which seeks to evaluate perceptions and developing ideologies, it is always difficult to prove conclusions, but she believes she has accumulated enough literary evidence to make her point. She is such a careful scholar that she allows evidence that tempers her case to come into play. For example she notes that Upper Canadians, in spite of the links, did, indeed, feel threatened by the expansionist United States, and she further acknowledges that, after the War of 1812, a strong Tory critique of the United States emerged in Upper Canada and became an integral part of political culture. In this context then, her thesis is not a powerful rejection of the work of the 1960s, after all, since she, too, concedes the emergence of a strong anti-American element in early nineteenth-century Canadian political culture. Her account, however, provides us with a more multidimensional assessment of Upper Canadian values and attitudes.

Although Errington does a first-rate job with her Canadian sources, her analysis could have been even more sophisticated had she looked more closely at recent work on Anglo-American political culture in the eighteenth century. For example she locates Canadian political culture somewhere between the British and American patterns, but the Canadian constitution of 1791 and the political views of the men who occupied the top ranges of the system that developed from

that constitution were much more conservative and exclusive than the workings of the contemporary constitution in England. This is a particularly interesting point because, in his iconoclastic book *English Society, 1688–1832* (1985), J. C. D. Clark uses the Canadian constitution of 1791 to illustrate the vitality of conservative values in late eighteenth-century England. This meant that Upper Canada, rather than lying between the United States and Britain in terms of political culture, had a more extreme Tory cast. It was the confrontation between this extreme construct and North American settler society that led to the explosion of 1837.

Still, the fact that such issues can be raised from this work is confirmation that Errington's thoughtful analysis has focused attention on key matters and has provided a more nuanced understanding of early nineteenth-century Upper Canada.

GORDON T. STEWART
Michigan State University

DAVID C. JONES. *Empire of Dust: Settling and Abandoning the Prairie Dry Belt*. Edmonton: University of Alberta Press; distributed by University of Nebraska Press, Lincoln. 1987. Pp. xiv, 316. Cloth \$24.95, paper \$14.95.

That part of the Palliser Triangle astride the Alberta-Saskatchewan boundary with Medicine Hat as its hub is the empire of this book's title; its temporal span the first three decades of this century. Through a combination of interviews, archival research, and empirical data, David C. Jones traces during this short era the settling and unsettling of one of North America's least hospitable regions. Although his searing account of false promises, vanquished hopes, and misguided policies is not unique in the chronicles of agricultural settlement, it is original in two respects: the skillful interweaving of the literary and academic approaches it employs and the place and time it studies.

Balancing meteorological, demographic, and agricultural data (summarized in an indispensable appendix) against myriad firsthand descriptions of the period, Jones depicts the (ultimately) hopeless struggle of a single generation of dryland farmers. Against a backdrop of official ineptitude and natural disasters of biblical severity—fires, blizzards, drought, and pests—the calamity of the region is recounted through the misfortunes of one community, Carlstadt, and its inhabitants.

Although most writers on western Canada have cited the economic recession that heralded the 1920s, none has until now extracted the local disaster buried within the statistics. In the Palliser

heartland, however, the Roaring Twenties were not the soft prelude to a decade of drought and depression but rather a bitter taste of what the prairies generally were soon to experience. Indeed, according to Jones, the devastation and dislocation experienced there between 1921 and 1926 were never to be surpassed: "A fifth [to] a quarter of all townships in southeastern Alberta . . . [lost] at least 55 percent of their population" (p. 117). The juxtaposition of documented fact and personal reminiscence lends vividness and authenticity to this and other accounts of agricultural collapse and human misery.

In addition to its graphic descriptions, the book develops two strong subthemes: one about the causes of the calamity, the other about attempted remedies. The apostles of scientific agriculture (among them W. R. Motherwell, Saskatchewan and later federal minister of agriculture) are condemned for their proselytization on behalf of practices such as summer fallowing, which encouraged settlement of an area perennially short of moisture, and politicians and servants of the United Farmers of Alberta government who sought solutions to the disaster are praised. More than praised, for the author indicates that, in their response to this concentrated and intense collapse of a region, they planted the seeds of Alberta's modern rural society through the introduction of larger school units, county government, and special districts. These last, which encompassed the heart of this "empire," provided the locale for the next (and this time federal) attack on the problems of the Palliser Triangle: the Prairie Farm Rehabilitation Act. That is another story to which this book offers a singularly harrowing prologue.

DAVID E. SMITH
University of Saskatchewan

MARLENE SHORE. *The Science of Social Redemption: McGill, the Chicago School, and the Origins of Social Research in Canada*. Buffalo, N.Y.: University of Toronto Press. 1987. Pp. xviii, 340. Cloth \$37.50, paper \$16.95.

Marlene Shore has written that rare monograph that would have been even better had it been longer, a sharp contrast to the books containing articles inflated to increase significance and preferment. Through careful exposition and illuminating brief commentary, Shore casts light on a number of large subjects important to any historian of ideas and academic institutions, in particular the transfer of knowledge and viewpoints from the United States to Canada and the importance of context, both intellectual and institutional, for the development and decline of ideas.

In concrete terms the book gives a good account of the Chicago school of sociology and the overlapping but distinct Canadian context of social work and applied moral philosophy that the Chicago school entered via the migration of Everett Hughes and, especially, the Canadian Carl Dawson to Montreal in the 1920s. Shore's account of Dawson has ideal-typical significance: a Baptist who made the pilgrimage to Chicago to study divinity, he found the pragmatic psychology and sociology taught there such a powerful application of idealism to the secular world that he emerged as a sociologist with a special interest in religion. Many Americans followed similar paths; indeed, one effect of Shore's account is to "bracket" the Forty-ninth Parallel, to show the common cultural milieu that then made Chicago theory relevant for some Canadians. It may have been the openness and eclecticism of Chicago theory, as well as its base in English thought—Adam Smith, Charles Darwin, and Herbert Spencer—that made it attractive to Canadians in a way that the next generation of American sociology, influenced by Sigmund Freud, Max Weber, and Emile Durkheim via Talcott Parsons, did not achieve. But, of course, much else had changed, and Shore, writing within a more nationalistic context, is anxious to build evidence for the case that Chicago sociology was not an alien transplant, imposing "American" perceptions on a different culture. She cites the origin of several Chicago teachers in Canada, and the strong influence on Chicago of a Protestant tradition of humanitarian social surveys common to the English-speaking world. True, but I suspect that this argument reflects the power of nationalist categories in the 1980s rather than the concerns of the 1920s, when a sense of fellowship among the Anglophone educated classes combined with the belief in an attainable scholarly "objectivity" to make ideas seem universal rather than limited to particular political contexts.

The book does, however, reveal a contrast between Chicago and Montreal, although it is largely a difference in the institutional independence of university faculty. The Scots-Canadian business elite of Quebec exercised far closer control over McGill than the Rockefellers did over Chicago, and, when in the 1930s fears of revolution rose, even an apolitical concern for investigating the condition of the masses came to seem a threat to social stability. No great cultural difference was involved, except, arguably, the sense of vulnerability, of constant threat, that has characterized Canadian elites exposed to the corrosive power of distance from their spiritual homes and proximity to vulgar democracy to the south. There was, however, an intellectual aspect to the attacks on research sociology. The harshest critic of empiri-

cal sociology inside McGill was the economist Stephen Leacock, now a celebrated cultural icon cherished for his humorous essays on provincial life. Leacock seems to have viewed a proper academic discipline as one containing a structure of permanent truths that could be taught didactically, like algebra or classical economics but unlike the loose combination of evolutionary theory with empathetic data gathering that Chicago sociology fostered. One is tempted to trace Leacock's intolerance to the difference between English utilitarianism and American pragmatism, but it expresses a tension always present in any academic field: what is the proper mix of humility and dogmatism, of questing for new insight with confident affirmation of the known? An American teaching in Canada is inclined to say that the expectation of dogmatism is stronger north of the border, but this dilemma imposed by two conflicting definitions of the university's role is universal. It is a sign of the value of this excellent book that it raises issues such as this without losing sight of its prime goal to illustrate the complex interactions between theory, personality, and institutional context. The idea of tracing ideas through institutional interaction could be applied within single national contexts: who will give us a study of the connections between Harvard and Berkeley?

FRED MATTHEWS
York University

LATIN AMERICA

JOHN A. BRITTON. *Carleton Beals: A Radical Journalist in Latin America*. Albuquerque: University of New Mexico Press. 1987. Pp. xiii, 309. \$24.95.

Carleton Beals served as a leading journalistic commentator on Latin America for five decades between 1918 and the 1960s. From the first he directed an idealistic critique at Latin American dictators while generally defending liberals, nationalists, and socialists. A left-wing populist, Beals continued to support progressive political movements in Latin America until the end of his career. His audience was the newspaper and magazine readership of the United States.

John A. Britton does a commendable job of capturing not only the Beals of the written word but also, it would seem, the essence of the man. Using interviews with Beals, his friends, and family; archival sources, including the records of the Central Intelligence Agency and the Federal Bureau of Investigation; and Beals's impressive assortment of essays, the author describes a tumultuous and productive life. He does so in an unpretentious and readable style. Beals is placed in his time and context.

At the beginning of America's involvement in World War I, Beals was a student at the University of California at Berkeley. His parents were radicals, and Beals found Berkeley's political milieu to his liking. He supported Eugene Debs's candidacy for the presidency of the United States, resisted the draft, and went to jail briefly. In 1918, when new elites were consolidating a far-reaching social revolution in Mexico, he went to that country. He developed a cosmopolitan and interesting circle of friends including Diego Rivera, Bertram Wolf and Ella Wolf, and, later, Josephus Daniels. Early on, he identified Luis Morones as corrupt and a murderer and saw the need to protect Mexico's rural social fabric. In his most important book, *Porfirio Diaz: Dictator of Mexico*, he correctly identified the Porfiriato as a time of foreign economic penetration and domination. He regarded *Mexican Maze* as his best book.

Beals followed the action wherever it took him, and in 1928 that meant Nicaragua. He interviewed and rendered a favorable portrayal of Augusto Sandino at a time when the American press was generally labeling the Nicaraguan resistance leader a "bandit." In Cuba in 1933 he vehemently attacked U.S. Ambassador Harry F. Guggenheim for supporting the murderous regime of Gerardo Machado. He also visited Peru in 1934 where he interviewed APRA leader Victor Raul Haya de la Torre. He denounced the Good Neighbor policy as an ill-disguised instrument of U.S. expansionism.

In the early 1940s Beals suffered one of his more important defeats at the hands of American censorship. The Houghton Mifflin Company refused to publish his manuscript "Rio Grande to Cape Horn" until he removed criticisms of Caribbean dictators Rafael Trujillo and Fulgencio Batista. Houghton Mifflin then praised itself in advertising for having published an "utterly honest" book (p. 196).

Beals's continuing support of Latin American left-wing causes and his cochairing of the Fair Play for Cuba Committee led to secret political scrutiny by the American government during the McCarthyite era and the early 1960s. In one report the FBI libeled him as a "pervert." He published six books in the 1960s, but his popularity was fading. Carey McWilliams saw McCarthyite pressures as the cause of his decline. The author of thirty-four books and two hundred articles, Beals was unable to publish his work in the 1970s. He argued the legitimacy of revolution in Latin America and the illegitimacy of imperialism until his death in 1979.

JOHN MASON HART
University of Houston

ROBERT DIRKS. *The Black Saturnalia Conflict and Its Ritual Expression on British West Indian Slave Plantations*. (Social Sciences Monograph, number 72.) Gainesville: University Presses of Florida. 1987. Pp. xvii, 228. \$18.00.

Plantation slavery in the British Caribbean was one of the most demanding and brutal systems of exploitation found in the New World. Slaves worked extremely long hours at heavy labor, suffered fearful mortality, were subject to rigid discipline, and were employed in the highly regimented work of their masters on most days of the year. But every Christmas, for two or three days, permissiveness reigned. Slaves abandoned their work and wandered from plantation to plantation or into the towns. They adorned themselves in finery and took part in a tumultuous gala, dancing in the great houses of the planters, demanding and receiving food and drink. John Canoe, Actor-Boy, Set-Girls, and Moco Jumbo performed in the streets, dressed in elaborate costumes, openly satirizing their masters. The existence of this temporary license within such a tightly managed system of slavery, with a history of large-scale rebellion, raises fundamental questions about the nature of slave culture and the internal relationships of the larger socioeconomic structure. It is this apparent paradox that Robert Dirks seeks to explain in this study.

Dirks identifies his method as that of the cultural ecologist. What is necessary for this ethnological task, he contends, is a synchronic synthesis of the institutions of plantation society, not a historical synthesis. Dirks is interested in origins, certainly, but his answers come from a study of the plantation system operating as a cultural whole rather than from any discovery of roots. His initial description of the Christmas festivities, principally in the last decades of slavery, is very brief. Following chapters explore the physical and technical aspects of the plantation, the ecological niches of masters and slaves, and slave subsistence and mortality patterns. Dirks finds the focus of the plantation ecosystem in food. Competition for scarce resources led to a "scramble" or small-scale conflict within the system. Conflict also appeared in the use of magic and in open rebellion. After treating these themes, Dirks returns to the Christmas saturnalia. He characterizes the behavior of the slaves as agonistic rites, prompted by the sudden release of energy drawn from an unusually high intake of food in a situation of chronic malnutrition. He also notes the relatively high frequency of rebellion in the British West Indies during the month of December.

The historical data presented by Dirks are generally familiar. But he organizes them in a creative

manner, with an explicit theoretical objective, and so leads us to see competition and conflict in place of simple oppression and street theater. This enriches our vision of slave culture. Analytical difficulties arise, however, from Dirks's assumptions that British West Indian slave society was "overwhelmingly homogeneous" and that it is therefore safe to weave the "discrete and more or less incomplete descriptions from all over the British Caribbean into a more comprehensive whole" (p. xiv). Why, for example, were the agonistic rites of Jamaica or even Nevis so much more vital than those of Barbados? Nutrition and the regime of plantation labor, the variables emphasized by Dirks, seem capable of throwing light on this significant question, but he appears barely to recognize it. Sacrificing spatial and temporal variety to a synthetic vision may conceal as much as it illuminates.

There are some unfortunate lapses in the rendering of names: William Beckford appears as George Beckford on pages 21 and 28, John Luffman as William Luffman on page 41, H. T. De La Beche as William De La Beche on page 52, and Renny as Renney on pages 141 and 145. All of these are named correctly in the bibliography and elsewhere in the text. These blemishes detract from an essentially original and challenging contribution to the literature.

B. W. HIGMAN
University of the West Indies

JOHN FREDERICK SCHWALLER. *The Church and Clergy in Sixteenth-Century Mexico*. Albuquerque: University of New Mexico Press. 1987. Pp. xvi, 263. \$27.50.

The intent of this book is to redress the emphasis that historians have put on the role of the regular clergy in their writings on the church in sixteenth-century Mexico. This volume essentially presents the data from a study of the secular clergy of the *gobiernos* of Mexico and Nueva Galicia from the improvisations of the conquest to the mature state of the church hierarchy. John Frederick Schwaller launched research on this topic a decade ago in a doctoral thesis, and parts of this study are available in articles and papers.

The ample material, which is the heart and major contribution of the book, was gathered in a prosopography of the lower clergy. It traces the origin and career of secular priests through the records of *relaciones de parte y de oficio*, through examination lists, and through legal and financial documentation. The secular priests are distinguished from the regulars primarily by their social role and dependence on church governance un-

der the *patronazgo* and the viceroy. The selections from the large roster of minibiographies make lively reading and furnish indispensable detail in a field that has so far suffered from generalization without control of fact. Although the overall conclusions hold no surprises, the detail is invaluable. Editorial care has been indifferent, overlooking unevenness of style and a crop of misprints. The index is reliable, and the bibliography reflects the state of the subject.

Roughly the first quarter of the book serves as an introduction to clarify the difference between the *de jure* appearance of church organization and the *de facto* operation of the institution in the colonies. This discussion opens with a chapter on the "upper clergy," bishops and cathedral chapters. It describes their responsibilities, powers, compensations, and the state of the bishoprics. The careers of selected individuals also help illustrate the development of the colonies, for instance, in the cases of appointees who regarded a promotion in rank to the chapter of Oaxaca as an undesirable demotion to a poor bishopric serving an uncongenial provincial society.

The lower clergy that is the main subject of the book, parish priests and chaplains, comes to life in accounts of their varied duties and career patterns. Many priests worked outside of the church and had checkered careers, often depending on their ethnic and social origin. Their appearance before the bar in secular matters sheds light on the working of the judicial system of the colonies.

Three stages of institutional development of the church are established. The organization of the secular clergy began in earnest as the second phase after the conquest, with the establishment of dioceses, the introduction of the *ordenanzas del patronazgo*, and three church councils, (1555, 1565, 1585). After the visitation of the Consejo de Indias by Juan de Ovando (1569-71), the "mature" institution of the secular hierarchy remained in essence unchanged for the rest of the colonial period. This book, indispensable for its topic, is a valuable data base for historians of colonial Mexico.

URSULA LAMB
*University of Arizona,
Tucson*

SERGIO ORTEGA, editor. *De la Santidad a la Perversión: O de porqué no se cumplía la ley de Dios en la sociedad novohispana*. Mexico City: Grijalbo. 1986. Pp. 290.

Sergio Ortega has edited an entertaining, informative, often insightful collection of occasional essays that tell of such matters as the childhoods of

the devout, the Inquisition's suppression of French tales of love (such as Abelard and Heloise) from circulation, the solicitation of sex in the confessional, a controversy over a ritual dance before the statue of Saint Gregory, the rationalization of theft because of financial burden of a mistress as well as a wife, and bigamous marriage chosen in preference to fate at the hands of the vengeful relatives of a young woman seduced under promise of marriage. Each essay is built around vignettes derived from Inquisition records in eight of the essays and from civil records and published materials in three others.

The book opens with Sergio Noriega's learned essay on the diversity of opinion among sixteenth-century Mexican theologians about the status of Indian marriage and kinship rules after the conquest. He illustrates how the official European Catholic conception of marriage, consisting of free individual consent, was reinforced by contrast with Indian practices stressing parental authority over marriage.

Ana María Atondo Rodríguez reveals the Inquisition's inability to eliminate heterodox beliefs about sex even among prominent European-born Spaniards who held that intercourse between unmarried people was either not a sin or one of little gravity. Such beliefs thrived given the relatively light penances commonly handed out by parish priests. She further suggests that the Inquisition's prosecutorial role became more limited after the 1630s, as the Inquisition came merely to handle cases that private parties brought before it rather than pursuing cases on its own initiative as before.

Serge Gruzinski recounts a massive investigation by the Holy Office into homosexuality resulting in the burning at the stake in 1658 of 16 men (out of 123) accused of homosexual behavior. Although most of the accused (and all of those burned to death) were ordinary artisans (tailors, shoemakers, glovemakers), servants, slaves, and seven laborers in a single workhouse (*obraje*), the secret investigation of the more prominent—two clergymen and the son of a Puebla town councilman—enabled the viceroy to declare, perhaps self-deceptively, that the accused homosexuals were mestizos, Indians, blacks, and mulattoes, the "filthy rabble of the kingdom" (p. 271).

Solange de Alberro creates a sympathetic portrait of an intelligent, ambitious, but poor Spanish woman, Teresa Romero, who gained a reputation for saintliness both to enrich herself and to gain her father's affection. Originally creating a partially self-deceptive story of visions, possessions, fasts, and stigmata, Teresa gradually turned to a more cynical manipulation of her visions and possessions. Her portrayal of this role, de Alberro observes, succeeded by conforming to the implicit

expectations of her sex; she acted like a two-year old child since "woman, considered by Christianity as an imperfect creature relative to man, could not experience the divine presence except in a state of simplicity, passivity, and irresponsibility" (p. 225). Teresa's inability to sustain the role became apparent when someone who watched her maintain a state of ecstasy in the living room for an hour was disturbed to hear the raucous laughter in the kitchen to which she retired. The kitchen door opened to reveal the alleged holy woman leaning over a candle to light her cigarette. Portraits such as these make this collection a delightfully informative selection of research in the *histoire de mentalité* in Mexico.

PATRICIA SEED
Rice University

ERIC JAUFFRET. *Révolution et sacrifice au Mexique: Naissance d'une nation (1910–1917)*. (Sciences humaines et religions.) Paris: Cerf. 1986. Pp. 317.

This essay on the Mexican revolution is both peculiar and stimulating for it applies to the subject the myth analysis that René Girard successfully carried out regarding other questions (*La violence et le sacré* [1972]; *Le bouc émissaire* [1982]). A reader of Carl Jung, Mircea Eliade, and Girard, Jauffret has tried to apply their analytic process to the Mexican revolution. The result of this attempt was a doctoral thesis supervised by François Chevalier, a great specialist in Mexican history. Although Chevalier did not agree with all Jauffret's conclusions, he was of great help to him in having this work published.

According to Jauffret's study, the revolution of 1910–17 is the myth that has formed the basis of the Mexican nation in the twentieth century. And, if this element is not considered, socioeconomic theories, as well as linear causality, prove to be unsatisfactory. Emphasizing every element of mythical nature, Jauffret analyzes the debates of the revolutionary meetings, the political declarations, and the correspondence between members of the revolutionary elite.

The first part of his study deals with the discourse of the northern revolution and its many aspects, such as anarchism, jacobinism, socialism, and, especially, liberalism whether antiericlerical or not. The second part analyzes the discourse of the agrarian revolution and its close relationship to the mythic and the sacred. The third part is an attempt to establish a typology of revolutionary heroes by gathering them together in a group described as "heroic epiphany." Indeed, the triad of heroes-martyrs Francisco Madero, Pancho Villa, and Emiliano Zapata first gave to the revo-

lution a mythical orientation that definitively marked its development and meaning. These three heroes were actually mythic figures that were at the same time associated with other myths that they incarnated (Madero with democracy, Villa with war against injustice, Zapata with land tenure); they have turned into key elements of the Mexican consciousness in the twentieth century.

The final part is the most peculiar one. It consists of a study on sacrifice and its function in the genesis of the Mexico of today. This explains the subtitle of the book, "The Birth of a Nation." Revolution means violence, but in Mexico that violence, Jauffret says, is associated with the sacred and, thus, with sacrifice. After analyzing the Mexican revolution in two hundred thirty pages, the author devotes fifty more to the idea of sacrifice throughout Mexican history, from pre-Columbian times until today, combining the opinions of Octavio Paz on celebration, violence, and homicide with the concepts of Girard in search of continuity from the Aztec sacrifices to the violence of the revolution. The Aztec sacrifice was prohibited by the evangelization, he says, but, four centuries after the break resulting from the conquest, something of it still remains in the Mexican collective subconscious. In this study, which can be criticized but is worth reading, continuity prevails over discontinuity, as does phenomenology over causality, which, according to the author, does not provide the means of decoding the meaning of sacrifice and violence in Mexico.

JEAN MEYER

Centro de Estudios Mexicanos y Centroamericanos
Mexico City

KENDALL W. BROWN. *Bourbons and Brandy: Imperial Reform in Eighteenth-Century Arequipa*. Albuquerque: University of New Mexico Press. 1986. Pp. xii, 319. \$29.95.

Kendall W. Brown's offering to the burgeoning field of Bourbon studies is at once an appreciable contribution to the domain and an example of what premature publication can mean for promising scholarship. The focus of this work, its veritable climax, is the antitax rebellion of 1780 in Arequipa, Peru. The author tries to cast the movement in terms both of the regional concerns peculiar to southern Peru and of the broader imperial perspective. In this latter regard, however, the work is severely flawed. Indeed, the case for the place of this rising in the pattern of contemporary Andean tax revolts and indigenous rebellions is not strongly made. Further, his account of the constitutional attitudes of the citizenry is highly derivative of the work of John L. Phelan on the

comuneros, yet without demonstrating through conclusive documentary evidence that the people of Arequipa shared the notions of those of New Granada.

Brown is not particularly convincing as regards local causes for the rebellion, which in his account do not rise above the banal. This is quite surprising, for the early chapters of this book hold out a good deal of promise. It seems that the fundamental problem with the work is that the research design centers on the notion of a Bourbon attempt "to reconquer the colonies, to reassert royal authority over . . . an area economically integrated into the empire yet on the political periphery" (pp. 4-5). Viewed in this light, a chapter on unsuccessful resistance seems very much *de rigueur*, but such received wisdom rarely survives a young scholar's attempt to come to terms with his own thesis. In this case, however, adequate time for reflection was not allowed. Yet, notwithstanding such problems, this book has substantially redeeming virtues.

Among the more striking of these is the author's judgment on the nature of the reform effort on both a regional and an imperial scale. His conclusion that "the monarchy failed to enlarge the colonial economic pie significantly" and that "fiscal exactions soon reached a plateau, above which they did not rise because of the lack of economic expansion" is compelling and applicable to most of the empire (p. 217). In fact the last few pages of the book provide as good a brief overview of the reform process as I have ever read.

The true strength of the book, however, lies in the "descriptive" material. After a first chapter devoted to a very general account of the Arequipa region and its place in the empire under Habsburgs and Bourbons, the author launches into a number of additional chapters covering the agricultural sector, mining and manufacturing, and trade. These are exceedingly valuable, both for their characterization of these activities and for their statistical foundations. Taken together, they are a truly impressive foundation for a full-scale economic history of the region in Bourbon times, which Brown (who seems to have a remarkable bibliographical grasp) seems superbly qualified to produce.

Following these economic chapters come four more dealing with social change in Bourbon Arequipa, the church, political and economic reform, and fiscal reform. Although the chapter on political and economic reform is commonplace, those on the church and fiscal reform contribute powerfully to our understanding of the regional economy, while that on social change explains a good deal about the kind of resentment that partly underlay the rebellion of 1780 and the sort of

social evolution that allowed the continuation of the reform effort after that momentous event. In fact the revolt seems to have been no more than an accident in the rise of a new elite descended from immigrants. It is unfortunate that the author should have given it pride of place.

Lastly, the author is to be greatly praised for his statistical material. This includes figures on production, fiscal returns, demography, prices, trade, income, and more complex analytical statistics. Although satisfactory long-term series were not always available to him, Brown managed to draw out a great deal of meaning from shorter ones. Although this work is unfortunately flawed, he clearly has a great deal to teach us about Bourbon Peru. We can only await his next effort with anticipation.

JACQUES A. BARBIER
University of Ottawa

JOHN HEMMING. *Amazon Frontier: The Defeat of the Brazilian Indians*. Cambridge: Harvard University Press. 1987. Pp. xi, 647. \$29.95.

John Hemming is the author of seven books and the editor of three others concerning the indigenous peoples of South America and their relations with those who subjugated them. *Amazon Frontier* is a sequel to *Red Gold: The Conquest of the Brazilian Indians, 1500–1760* (1978), which was lavishly—and deservedly—praised in this journal (*AHR* 84 [1979]:890–91). The present volume begins with the establishment of the exploitive regime of civil directors (1757) who replaced the Jesuits and other missionaries as supervisors of Indian communities and concludes with the founding of the Indian Protective Service (1910). Successive chapters, focused primarily but not exclusively on Amazonia as the title would suggest, provide graphic evidence of how the *Brasis* succumbed to conquest, lethal diseases (measles, smallpox, tuberculosis, and syphilis), and alcoholism, how they lost possession of their tribal lands and their cultural heritage, and why their population contracted by an estimated 50 percent during the period. Throughout the narrative the author's sympathies clearly lie with the aboriginal victims, and his aversions are directed against those who persistently, often callously, exploited them, including the so-called directors, unscrupulous politicians, greedy merchants, land speculators, cattle ranchers, rubber hunters, slavers, professional assassins (*bugreiros*), and missionaries, whether Catholic or Protestant. Pioneer anthropologists, particularly Karl von den Steinen, a few literary figures such as João Francisco Lisboa, Antônio Gonçalves Dias, and José Alencar, and the fabled army

engineer Candido Rondón earn favorable marks because of their profound concern for the Indians' survival. The text is accompanied by two useful appendixes—one a chronology, the other a compilation of sixty biographical sketches of persons who had contact with the aborigines during the period and wrote extensively about them—as well as a formidable bibliography, five indispensable, albeit cluttered, maps, and sixty photographs, some of which significantly enhance the text.

Like its predecessor, this book is based on an array of published sources. It, too, is written in a vigorous, lucid, sometimes passionate style. Yet the book is not quite up to the standard set by *Red Gold*. Its twenty-four chapters offer the reader vignettes of the experiences of particular Indian tribes and accounts of the travels of outsiders who intruded on their isolated existence. But they do not provide a satisfyingly coherent account of the Indians' defeat during the one hundred fifty years the volume spans. The early chapters are marred by repetitive passages, by a number of factual flaws, and by an extremely loose organization. Nevertheless, no one writing in the English language knows as much about the travail of the Brazilian Indians during the past five centuries as does Hemming. His volumes will long remain indispensable references for anyone concerned about the disappearance of the first Brazilians. One looks forward—but grimly—to what will likely be the final volume of this remarkable trilogy.

DAURIL ALDEN
University of Washington

LINDA LEWIN. *Politics and Parentela in Paraiba: A Case Study of Family-Based Oligarchy in Brazil*. Princeton: Princeton University Press. 1987. Pp. xxiii, 497. \$52.50.

Linda Lewin has produced perhaps the most serious book on Brazil's regional oligarchy in a decade. Combining her superb anthropological techniques in studying a variety of familial and kinship systems with painstakingly careful and thorough archival research, she has written the best book ever in any language on Paraíba politics under the Old Republic (1889–1930). There is no doubt that her book is also the longest book on Paraíba history. Unlike earlier regional histories written by U.S. scholars, Lewin's book evolves around a single family, the Pessôas of Paraíba, and its satellite groups, or "factional groups," which Lewin defines as an oligarchy (pp. 26–27).

The strength of her work lies in a deft analysis of the Pessoa family-based networks in Paraíba

and its neighboring state of Pernambuco, especially the examination of the state and local political processes. By northeastern standards the Pessoa oligarchy was neither old nor rich before 1919, when the titular of the group was elected president of Brazil. Epitácio Pessoa began his political career under his uncle's auspices during the politically tumultuous 1890s, but the mentor and uncle, the Barão de Lucena, soon lost power both nationally and regionally. Epitácio became a member of Paraíba's "out" group led by former governor Venâncio Neiva. For a decade or so, Epitácio's political career seemed to go nowhere.

There were two national events that ameliorated the political fortunes of the Pessos. The nation-dividing presidential election of 1910 and subsequent reprisals on the northeast's losing oligarchies by the "salvationist" group realigned Epitácio to the victorious side in 1911. In 1919 Brazil's kingmakers (São Paulo, Minas Gerais, and Rio Grandado Sul) were unable to choose one of their own to succeed the president-elect, who died before taking office. Epitácio, who was out of the country at the time, became their compromise choice and was elected president in absentia. The access to federal coffers, resources, and patronage turned the Pessos into the *primus inter pares* of the state oligarchies.

Although Lewin's treatment of Paraíba politics is convincing, her handling of concepts and Brazil's general historical facts is not. While she is critical of those misusing the anthropological concept of "clan" (pp. 129–33), she is less than careful in adhering to the original meaning of the concept of "bourgeois" by referring to backland landowners as "agrarian bourgeois." In other places she treads on weak facts: her figure on the post-1870 slave population ("more than two million," [p. 184]) is inflated; one Albino Meira is presented as an imperial senator (p. 220) and so is Joaquim Nabuco (p. 122, n. 60). Her facts on the 1920s are no better: Juarez Távora, a well-known military rebel of the 1920s, is made a native of Paraíba (p. 350), and both Távora and Luis Carlos Prestes are considered "veterans" of the rebellious Copacabana Fort (p. 350). And she often uses the republican office names in describing those under the monarchy. At least in one place, she even refers to João Pessoa as Paraíba's capital during the period under study.

More questionable are her assertions that Brazil's (and Paraíba's) cousin (consanguineous) marriages are attributable to the "shortage of white women" (p. 147) and that the rise of exogamy was associated with the family's desire to preserve political influence (p. 177). Such generalizations are too sweeping and at times misleading. Perhaps, had Lewin looked at the broad waves of

social and economic changes that affected Paraíba and beyond, she might have given other reasons. This book is one case where a superb writing style has made Paraíba appear as a São Paulo of the northeast.

EUL-SOO PANG

Colorado School of Mines

JOSE ENIO CASALECCHI. *O Partido Republicano Paulista: Política e Poder (1889–1926)*. (Leituras AFINS.) São Paulo: Brasiliense. 1987. Pp. 325.

The Partido Republicano Paulista (PRP) dominated São Paulo politics during Brazil's Old Republic (1889–1930). Its leaders, members of an oligarchy who spoke of liberal ideals, were frequently in disagreement. Even before the monarchy fell, the "evolutionists" were criticized by a minority group that favored republicanism by revolution. Later, the party's internal rifts pitted "historical" Republicans against monarchists who joined the party after 1889, sound money advocates against proponents of coffee price support schemes, and state presidents against dissidents complaining of tyranny.

José Enio Casalecchi, in the narrative section that makes up 70 percent of his study, devotes most of his attention to these squabbles. He also shows how the party used a policy of accommodation with the central government when federal intervention in the state seemed imminent. It is a narrative carried out with an efficiency that leaves the reader occasionally wishing for more light about individuals, terms, and events. Joseph L. Love's immensely clarifying *São Paulo in the Brazilian Federation (1889–1937)* (1980), not to mention other well-indexed works such as Afonso Arinos de Melo Franco's *Rodrigues Alves* (1973), can be consulted with profit. Casalecchi's twenty pages listing obscure leaders in São Paulo's approximately one hundred seventy municipal districts (copied from issues of the *Correio Paulistano*) might better have been used to give his book an index.

The extent of the party's domination of the state is made clear in the useful chapter on election legislation and party organization. In 1906 dissidents may have hoped that the introduction of nominating conventions and the division of the state into ten electoral districts would weaken the party's central commission, which was enlarged from five to seven members. But the commission, if it bothered about perfunctory consultations at the lower levels, merely turned to men who owed their positions to it. And so it continued unhampered in naming judges and legislators as well as the state's presidents and representatives in Congress. Methods for retaining power—violence by

the police and ruffians, along with electoral fraud—are presented to the reader in the form of quotations from the dissidents' *O Estado de S. Paulo*.

Turning to what he calls the decline of the PRP in the 1920s, Casalecchi argues that the social question was becoming important by 1926, to the detriment of the São Paulo elite (Democratic party leaders as well as Republicans), who scorned the common people and rejected the views of anarchists, Communists, and other radicals. For those who feel that the decline of the PRP was largely determined by the decisions it made that affected relations among the Brazilian elite, in São Paulo and elsewhere, this part of the discussion may seem incomplete. Casalecchi, however, has no doubt achieved his purpose of presenting much information about the party before 1926.

JOHN W. F. DULLES
University of Arizona

FREDERICK C. LUEBKE. *Germans in Brazil: A Comparative History of Cultural Conflict during World War I*. Baton Rouge: Louisiana State University Press. 1987. Pp. xiii, 248. \$32.50.

Frederick C. Luebke has followed his *Bonds of Loyalty: German Americans and World War I* (1974) with a parallel volume concerning "the German ethnic experience in Brazil in the era of World War I" (p. 3). Certainly the parallels seem striking. From the 1820s to World War I, Brazil, like the United States, received a large European immigration in proportion to its population. Although immigrants to Brazil were predominantly Italian, Portuguese, and Spanish in origin, among ethnic contingents the Germans were in fourth place; there were an estimated four hundred thousand German speakers in Brazil in 1914 (the nation's total population was 30.6 million in 1920). When Brazil followed the United States into war against the Central Powers in October 1917—the only major Latin American nation to do so—the Teuto-Brazilians, like the German Americans before them, were plunged into a multidimensional crisis.

The book's subtitle notwithstanding, the author's treatment is descriptive, for he feels that before he can draw meaningful contrasts he must "present basic historical information about German immigrants in Brazil and their place in Brazilian society" (p. 3). Making use of materials in the Hans Staden Institute in São Paulo, the Institut für Auslandsbeziehungen in Stuttgart, and (for the Luxburg Telegram Affair) the U.S. National Archives, he has done this in three background chapters, five chapters on the war years, and an "Aftermath and Retrospect."

The first German immigrants were farmers and soldiers brought by Dom Pedro to Rio Grande do Sul in the 1820s to counteract the centrifugal proclivities of the area's large landholders. Through the nineteenth century German settlers continued to sift into the southern states of Rio Grande do Sul, Santa Catarina, and Paraná; ascending the rivers, they formed compact German-speaking zones on the agricultural frontiers. The numbers crossing the Atlantic were not great, for the shortcomings of the Brazilian sharecropping system, legal disabilities imposed on Protestants, and the generally baneful effect of slavery on free labor placed Brazil at a great disadvantage vis-à-vis the United States and other immigrant countries. Nevertheless, farm wives proved remarkably fecund; natural increase caused the German enclaves to expand and imprinted Teutonic culture on many of the small towns of the south.

Germany's resort to unrestricted submarine warfare in early 1917 shook Brazilian neutrality and ultimately brought war. The government responded to the sinking of the *Paraná* on April 5 by breaking diplomatic relations and impounding forty-three German merchant ships then in Brazilian ports; rioters attacked German businesses and clubhouses in Porto Alegre; Allied agents circulated bizarre rumors of clandestine preparations for an uprising of the Teuto-Brazilians. The slowness of the subsequent drift to war was attributed—unfairly, in Luebke's view—to the pro-German sympathies of Foreign Minister Lauro Müller. In September the U.S. government published intercepted messages from the German ambassador in Buenos Aires, Count Karl von Luxburg, that underlined the ruthlessness of German naval policy toward the South American states. The American move failed to push Argentina into war but succeeded in Brazil. When war was declared at the end of October, German businesses and institutions suffered a second round of riots more devastating than the first. German schools and the German-language press were shut down for the duration. The impounded ships and a seat at the peace table were Brazil's prizes of war.

Evidently, the English-language readership for which the book is intended is presumed to be unfamiliar with Brazilian history, the extensive Portuguese, French, and German historiography concerning the Teuto-Brazilians, or the general literature on the adaptation and assimilation of European and Asiatic immigrant groups in Latin America. It is regrettable that the author misses the latter, for he thereby misses numerous opportunities to develop fruitful comparisons—more fruitful than his passing, not very systematic, comparisons with the United States.

Indeed, the book disappoints on several accounts. It contains virtually nothing concerning the extent and structure of German economic interests in Brazil (which in 1914 were second only to Argentina among the Latin American states; after the war they would surpass them), hence nothing on the structure of German business relations with Brazilians and other Europeans, hence nothing on the economic aspect of Brazilian belligerence. Neither the occupational structure nor the social hierarchy of the Teuto-Brazilians is adequately described (the world of the cosmopolitan Hanse trader in Rio was not the world of the Paraná frontiersman, even though both were "Germans"). The account of the 1917 riots is far too long, yet the reader is not told who the rioters

were (students, middle-class patriotic societies, *Lumpen* paid by the Allied missions?). The suppression of German-language institutions is ascribed to "nativistic" pressures, about which we learn little. And, by cutting off his history in 1918, the author leaves important questions hanging—whether, for example, with renewed immigration in the 1920s German Brazil recovered its cultural autonomy or whether the consequences of World War I continued to be felt during the even more traumatic events of the Hitler era, when the autonomy of German Brazil was once and for all smashed by the hammer of Brazilian nationalism.

RONALD C. NEWTON
Simon Fraser University

Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

FRANCISCO O. RAMIREZ, editor. *Rethinking the Nineteenth Century: Contradictions and Movements*. (Studies in the Political Economy of the World-System; Contributions in Economics and Economic History, number 76.) New York: Greenwood. 1988. Pp. xix, 222. \$39.95.

FRANCISCO O. RAMIREZ, Introduction: What Is to Be Learned? RICHARD RUBINSON and JOAN SOKOLOVSKY, Patterns of Industrial Regulation: Railroads in the World Economy. THOMAS D. HALL, The Transformation of the Mexican Northwest into the American Southwest: Three Paths of Internal Development. PHILIP MCMICHAEL, The Crisis of the Southern Slaveholder Regime in the World Economy. GEORGE M. THOMAS, Cultural Contradictions and Religious-Political Movements in the Nineteenth-Century United States. JUAN MANUEL CARRION, The Origins of Puerto Rican Nationalism: Precocity and Limitations of the Nineteenth-Century Independence Movement. DALE TOMICH, The "Second Slavery": Bonded Labor and the Transformation of the Nineteenth-Century World Economy. MICHAEL S. KIMMEL, "Disheveled Improvization": Agrarian Resistance to Industrialization in the Late Nineteenth Century. NESAR AHMAD, Origins of Hindu-Muslim Conflict: Impact of the World Economic Crisis, 1873-96. RESAT KASABA, Up the Down Staircase: British Policy in the Near East, 1815-74. KATHLEEN C. SCHWARTZMAN, Portugal at the Neocolonial Fringe of the British Empire. IMMANUEL WALLERSTEIN, Should We Unthink the Nineteenth Century? JOHN W. MEYER, Society without Culture: A Nineteenth-Century Legacy. RAYMOND GREW, Rethinking the Assumptions and Purposes of Nineteenth-Century Social Science.

BRIAN ELLIOT, editor. *Technology and Social Process*. Edinburgh: Edinburgh University Press; distributed by Columbia University Press. 1988. Pp. xiii, 187. \$37.50.

TOM HUGHES, The Seamless Web: Technology, Science, et cetera, et cetera. BRUNO LATOUR, *The Prince for Machines*

as Well as for Machinations. JOHN LAW, The Anatomy of a Socio-Technical Struggle: The Design of the TSR 2. TREVOR PINCH, Understanding Technology: Some Possible Implications of Work in the Sociology of Science. CHRISTOPHER FREEMAN, Induced Innovation, Diffusion of Innovations and Business Cycles. MARTIN FRANSMAN, The Japanese System and the Acquisition, Assimilation, and Further Development of Technological Knowledge: Organizational Form, Markets, and Government. DAVID WOLFE, Socio-Political Contexts of Technological Change: Some Conceptual Models. DONALD MACKENZIE, et al., Social Research on Technology and the Policy Agenda: An Example from the Strategic Arms Race.

THOMAS F. GLICK, editor. *The Comparative Reception of Relativity*. (Proceedings of the Boston Colloquium for the Philosophy of Science, 1983; Boston Studies in the Philosophy of Science, number 103.) Boston: D. Reidel; distributed by Kluwer Academic, Norwell, Mass. 1987. Pp. vii, 412. \$79.00.

STANLEY GOLDBERG, Putting New Wine in Old Bottles: The Assimilation of Relativity in America. JOSE M. SANCHEZ-RON, The Reception of Special Relativity in Great Britain. LEWIS PYENSON, The Relativity Revolution in Germany. MICHEL PATY, The Scientific Reception of Relativity in France. MICHEL BIEZUNSKI, Einstein's Reception in Paris in 1922. BARBARA J. REEVES, Einstein Politicized: The Early Reception of Relativity in Italy. THOMAS F. GLICK, Relativity in Spain. V. P. VIZGIN and G. E. GORELIK, The Reception of the Theory of Relativity in Russia and the USSR. BRONISLAW ŚREDNIAWA, The Reception of the Theory of Relativity in Poland. TSUTOMU KANEKO, Einstein's Impact on Japanese Intellectuals. THOMAS F. GLICK, Cultural Issues in the Reception of Relativity.

C. W. KILMISTER, editor. *Schrödinger: Centenary Celebration of a Polymath*. New York: Cambridge University Press. 1987. Pp. 253.

C. W. KILMISTER, Introduction. DIETER FLAMM, Boltzmann's Influence on Schrödinger. JON DORLING, Schrödinger's Original Interpretation of the Schrödinger Equation: A Rescue Attempt. J. S. BELL, Are there Quantum Jumps? CHEN NING YANG, Square Root of Minus One, Complex Phases, and Erwin Schrödinger. W. W. THIRRING, Consequences of the Schrödinger Equation

for Atomic and Molecular Physics. MARTIN KARPLUS, Molecular Dynamics: From H+H₂ to Biomolecules. KENICHI FUKUI, Orbital Presentation of Chemical Reactions. A. D. BUCKINGHAM, Quantum Chemistry. WILLIAM MCCREA, Eamon de Valera, Erwin Schrödinger, and the Dublin Institute. J. T. LEWIS, Do Bosons Condense? JAMES MCCONNELL, Schrödinger's Nonlinear Optics. O. HITTAIR, Schrödinger's Unified Field Theory Seen 40 Years Later. S. W. HAWKING, The Schrödinger Equation of the Universe. A. SALAM, Overview of Particle Physics. T. W. B. KIBBLE, Gauge Fields, Topological Defects, and Cosmology. M. J. SEATON, Quantum Theory and Astronomy. LINUS PAULING, Schrödinger's Contributions to Chemistry and Biology. M. F. PERUTZ, Erwin Schrödinger's *What is Life?* and Molecular Biology.

BEHRAM N. KURSUNOGLU and EUGENE P. WIGNER, editors. *Reminiscences about a Great Physicist: Paul Adrien Maurice Dirac*. New York: Cambridge University Press. 1987. Pp. xviii, 297. \$49.50.

BEHRAM N. KURSUNOGLU, A Memorial to P. A. M. Dirac. BEHRAM N. KURSUNOGLU and EUGENE P. WIGNER, Preface. MARGIT DIRAC, Thinking of My Darling Paul. SEVDA A. KURSUNOGLU, Dirac in Coral Gables. JOSEPH E. LANNUTTI, Recollections of Paul Dirac at Florida State University. HARISH-CHANDRA, My Association with Professor Dirac. N. KEMMER, What Paul Dirac Meant in My Life. RUDOLF PEIERLS, Dirac's Way. A. D. KRISCH, An Experimenter's View of P. A. M. Dirac. HENRY KING STANFORD, Dirac at the University of Miami. EUGENE P. WIGNER, Remembering Paul Dirac. R. H. DALITZ, Another Side to Paul Dirac. A. PAIS, Playing with Equations, the Dirac Way. LAURIE M. BROWN and HELMUT RECHENBERG, Paul Dirac and Werner Heisenberg—a Partnership in Science. WILLIAM J. MARCIANO and MAURICE GOLDBABER, Dirac's Magnetic Monopole and the Fine Structure Constant. F. HOYLE, Magnetic Monopoles and the Halos of Galaxies. P. A. M. DIRAC, The Inadequacies of Quantum Field Theory. P. T. MATTHEWS, Dirac and the Foundation of Quantum Mechanics. J. C. POLKINGHORNE, At the Feet of Dirac. NEVILL MOTT, Reminiscences of Paul Dirac. HARRY J. LIPKIN, From Relativistic Quantum Theory to the Human Brain. J. WEBER, Dirac in 1962, Weak and Gravitational Radiation Interactions. WILLIS E. LAMB, JR., Schrödinger's Cat. ABDUS SALAM, Dirac and Finite Field Theories. BEHRAM N. KURSUNOGLU, Dirac's Influence on Unified Field Theory.

PETER J. HUGILL and D. BRUCE DICKSON, editors. *The Transfer and Transformation of Ideas and Material Culture*. College Station: Texas A&M University Press. 1988. Pp. xxii, 281. \$29.50.

GEORGE F. CARTER, Cultural Historical Diffusion. ALAN OSBORN, Limitations of the Diffusionist Approach: Evolutionary Ecology and Shell-Tempered Ceramics. D. BRUCE DICKSON, Anthropological Utopias and Geographical Epidemics: Competing Models of Social Change and the Problem of the Origins of Agriculture. WILLIAM H. MCNEILL, Diffusion in History. KARL W. BUTZER, Diffusion, Adaptation, and Evolution of the Spanish Agrosystem. PETER J. HUGILL, Technology Diffusion in the World Automobile Industry, 1885–1985. BRUCE SEELY, The Dif-

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JEFFREY C. ALEXANDER, editor. *Durkheimian Sociology: Cultural Studies*. New York: Cambridge University Press. 1988. Pp. xi, 227. \$39.50.

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EDWARD TIMMS and NAOMI SEGAL, editors. *Freud in Exile: Psychoanalysis and Its Vicissitudes*. New Haven: Yale University Press. 1989. Pp. 310. \$32.50.

SANDER L. GILMAN, Constructing the Image of the Appropriate Therapist: The Struggle of Psychiatry with Psychoanalysis. IVAR OXAAL, The Jewish Origins of Psychoanalysis Reconsidered. UWE HENRIK PETERS, The Psychoanalytic Exodus: Romantic Antecedents and the Loss to German Intellectual Life. EDWARD TIMMS, Freud's Library and His Private Reading. RITCHIE ROBERTSON, Freud's Testament: *Moses and Monotheism*. MURRAY G. HALL, The Fate of the Internationaler Psychoanalytischer Verlag. R. ANDREW PASKAUSKAS, The Jones-Freud Era, 1908–1939. PEARL KING, Early Divergences between the Psycho-Analytical Societies in London and Vienna. STEPHEN BANN, Adrian Stokes: English Aesthetic Criticism under the Impact of Psychoanalysis. FREDERICK WYATT, The Severance of Psychoanalysis from Its Cultural Matrix. ERNST FEDERN, Psychoanalysis—The Fate of a Science in Exile. MARTIN STANTON, Wilhelm Stekel: A Refugee Analyst and His English Reception. MALCOLM PINES, The Question of Revising the Standard Edition. RICCARDO STEINER, "Die Weltmachtstellung des Britischen Reichs": Notes on the Term "Standard" in the First Translations of Freud. DARIUS GRAY ORNSTON, JR., How Standard is the "Standard Edition"? ALEX HOLDER, Reservations about the Standard Edition. HELMUT JUNKER, On the Difficulties of Retranslating Freud into English:

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Documents and Bibliographies

The following collections of documents, bibliographies, and other similar works were received by the *AIR* between October 1 and November 16, 1988. Books that will be reviewed are not usually listed, but listing does not necessarily preclude subsequent review.

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ARTICLES

TO THE EDITOR:

This letter is intended as an additional contribution to your important *AHR Forum* on film and history (*AHR*, 93 [1988]: 1173–1227). That discussion centered on the possibilities and limits of presenting history or historical controversies accurately in films dealing with events that could be more fully known through reading. It seems important to note that there are whole areas of social history, particularly in relatively new fields such as gender, ethnic, and comparative history, in which classroom use of a well-chosen group of films can tell us much about history and society that is essentially inaccessible with books. Most social history is not concerned with the details of the lives of specific persons but rather with the patterns of the lives of groups of persons. The problem of accuracy does not arise in the same way as it does, for example, with Lyndon Baines Johnson. In many cases, historically informed fiction films can reveal more about social history than a factually based film that sticks strictly to the available record, since the fiction film, like the novel, can fuse the record of a number of cases. Also, a comparison of several films may shed light on comparative history in a way that cannot be duplicated in written history.

These reflections are aroused mainly by a course I am giving for the first time in 1989, "Gender Relations in the Mediterranean as Seen through Film." The idea for the course first came to me some years ago when I noted that many aspects of traditional gender relations as depicted in such films of the Christian Mediterranean as Michael Cacoyannis's *Zorba the Greek* and Pietro

Germi's *Seduced and Abandoned* corresponded very closely to what were widely considered to be Muslim customs—especially the stoning and killing of the young widow in *Zorba* and the horrendous treatment of a seduced virgin in *Seduced and Abandoned*. I began to look for other films that might have points to compare and contrast and also for readings that might make similar historical or anthropological points. With the films, I was quite successful but not with the readings. No one, to my knowledge, has done a significant comparative study of gender relations in the north and south Mediterranean, and even good books with promising titles, such as Monique Gadant's edition of *Women of the Mediterranean* (1986) or J. G. Peristiany, *Honour and Shame: The Values of Mediterranean Society* (1966), have precious little comparison and, in the latter case, surprisingly little about women.

I was thus faced with the need to rely on my general knowledge of history and film, and on scattered reading, but in the end, with the help of Terry North and Sherifa Zuhur, came up with a course in which the films themselves, supplemented by introduction, commentary, and discussion, bear the burden of the course. Its aim is not to give a detailed chronological history of anything but rather to show something of how gender relations functioned at different times and places in Mediterranean societies.

To mention some of the films I am using (one in each four-hour session), we begin with *The Return of Martin Guerre*, the most frequently discussed film in your *Forum*. None of your authors, however, mentions the point stressed in this course, namely, how crucial decisions taken by an externally obedient young woman can be. It was Bertrande's decision to recognize the false Martin that was decisive, just as it was her decision to delegitimize him at the end that sealed his fate. Although she says that she saw in his eyes that he wanted her to save her own life, she may have seen in those eyes what she wished to see. It is clear in the film that she was making momentous decisions

to better her position. The exact accuracy of events is less important than the broader truth, supported by numerous historical and anthropological studies in the Mediterranean and elsewhere, that women, even in societies more blatantly male-dominated than that of sixteenth-century France, are able to make many important decisions and shape many aspects of their lives despite codes of male dominance.

The next film I present in class is Moshe Mizrahi's *I Love You Rosa*. The gender implications of this film are suggested in a *New York Times* article, May 15, 1972. It notes that in the Cannes audience for this Israeli love story involving a thirteen-year-old boy and a twenty-one-year-old widow a century ago in Jerusalem: "Women . . . cheered and applauded the two actors . . . At an earlier screening to an almost entirely male audience, the film had been received with a mixture of groans and walkouts." The story is based on the life of Mizrahi's grandmother, who, by biblical law, as a young widow had to marry her eleven-year-old brother-in-law. She reared him and later married him. "Dealing as it does with the problems of a young woman in a totally male-dominated society, *I Love You Rosa* was as enthusiastically received by the younger militant feminists in the audience as by the older women." Clearly, the problems of male dominance and of regulation of marriage by religion have comparable resonance for Muslims and Christians.

Films from the Muslim Mediterranean include Sherif Gören's *Yol*, with its pitiless depiction of the negative aspects of traditional customs and gender relations in eastern Turkey; Michel Khleifi's *Wedding in Galilee*, dealing with a partly traditional wedding under the circumstances of Israeli control; and *The Battle of Algiers* by Gillo Pontecorvo, showing women's participation in a national revolution. These films run the gamut from the worst aspects of female submission and victimization through women's celebration and struggles. Parallels to many of these situations in the non-Muslim world are discussed in class.

Among these parallels are the brutal treatment of adultery and fornication, already noted for Sicily and Greece in *Seduced and Abandoned* and *Zorba the Greek*. These films, as well as *Yol* and *Martin Guerre*, lead to discussions of possible reasons for especially strict sex codes and punishments for adultery and premarital sex in Mediterranean societies. Each film chosen is rich in meanings and will provide many more topics for discussion than the few suggested here.

One non-Mediterranean film I was unable to resist using is the Senegalese film *Jom* by Ababacar Samb Makharam, a film just as outstanding as

Ousmane Sembene's *Ceddo*, also from Senegal, mentioned in your *Forum*. In this film, strikers are exhorted to the traditional spirit of *Jom* (unity, solidarity) by a brilliant *griot* who tells them oral histories of struggles against the French, and also of a woman singer who uses her performance to defend women servants against their upper-class masters. Past and present come together when women turn on their scabbing men and make them struggle once again. The blending of the past and present stories, and of gender, class, and anti-imperialist struggles, is brilliant.

The course ends with Jean Renoir's *Rules of the Game*, in which the formerly grave question of adultery would seem to be reduced to double farce—with the aristocrats now emulated by their servants. And yet the old codes are still strong enough that the consequences of adultery may still be what they were in stricter days.

Discussion of the films is accompanied by discussion of the general evolution of social practices in the countries where they originate. It is also important to show how many similarities there are and were among different cultures and religions. There is little doubt that an appreciation of the role of women, whether as victims, as actors, or both, can be seen in some of these films in ways that reading by itself cannot emulate. Until now, books have not dealt with the kind of comparisons that show up in this group of films.

There is nothing definitive about the films chosen. Several other films were not available, including Pietro Germi's *Divorce*, *Italian Style* and the marvelous but apparently unrecorded Spanish *Calle Major* by J. A. Bardem. (I substituted Luis Buñuel's *Viridiana*.) Some may fault me for including no women directors, but I could find no films by women that filled my general purpose.

The above course is only one of an infinity of possible courses in social history using fiction films. Certainly, courses on gender relations in modern America or modern Europe would be easy to put together and to find readings for. For decades, historians have been using novels in their courses without feeling a need to apologize for their possible historical errors. Fiction films can play much the same role.

Although fiction films were not the subject of your *Forum*, it sometimes read as if the exact factual accuracy of films was a more important topic than it is to teachers who wish to show how life was lived in other times and places.

NIKKI R. KEDDIE
University of California,
Los Angeles

REVIEWS OF BOOKS

TO THE EDITOR:

I was disturbed to see in David Underdown's review of J. C. D. Clark's *Revolution and Rebellion: State and Society in England in the Seventeenth and Eighteenth Centuries* (*AHR*, 93 [1988]: 1047–48) the implication that all Early Stuart and Hanoverian "revisionist" historians share the same (right-wing) politics as Dr. Clark, and that this colored Professor Underdown's opinion of the book. Whatever merits (or lack of them) the book may have, stereotyping of this kind is always unwise and may be actively pernicious, as it encourages students and other historians to regard divergent schools of interpretation to that favored by a reviewer whose politics they sympathize with as the property of some alien, enemy "Other." Also, it is simply inaccurate. Conrad Russell, the doyen of the Early Stuart revisionist school (who recently took up his peerage in order to oppose the British Conservative government's Educational Reform Bill), sits in the Lords as a Liberal and Social Democrat peer. My own politics are diametrically opposed to those of Dr. Clark, although I am happy to accept the epithet "revisionist" that he attaches to me. There is no rational reason why the acceptance of the theses that continuity needs more emphasis in the historiography of the seventeenth and eighteenth centuries or that the mass of the British people

remained politically inert and conservative (with a small "c") throughout the period should dictate a penchant for present-day right-wing politics.

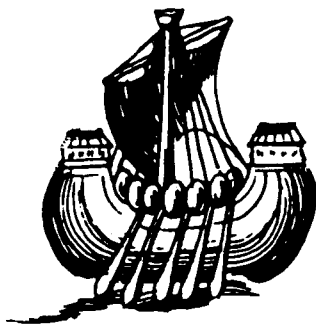
DANIEL SZECHI
Auburn University

PROFESSOR UNDERDOWN REPLIES:

I am well aware that not all revisionist historians share J. C. D. Clark's political stance, and my review certainly did not intend to imply that they do. Perhaps the restoration of the word "rightly" after "Clark" in line 6, page 1048, of my review would have made this clearer; it was there originally but got excised during the process of shortening the review to meet the editor's entirely proper standards of length. The relationship between any historian's text and his or her (political) subtext is often a complicated one and rarely is it as obvious as in Clark's case. As Professor Szechi may know, my own work pays a lot of attention to seventeenth-century popular conservatism (although my approach is very different from Clark's), and this does not make me a Thatcherite, either. Clark is much more inclined to make simplistic equations between historical interpretations and political attitudes than I am: I was simply reviewing his book.

DAVID UNDERDOWN
Yale University

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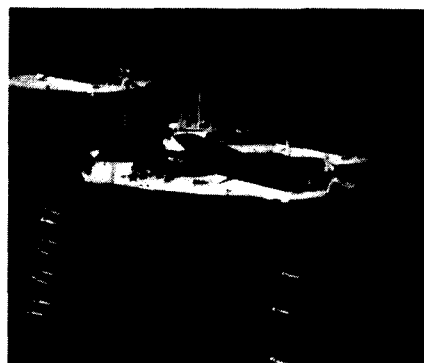
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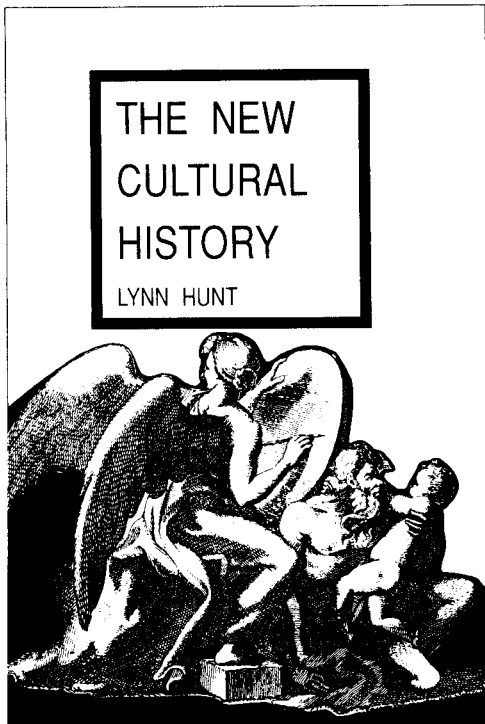
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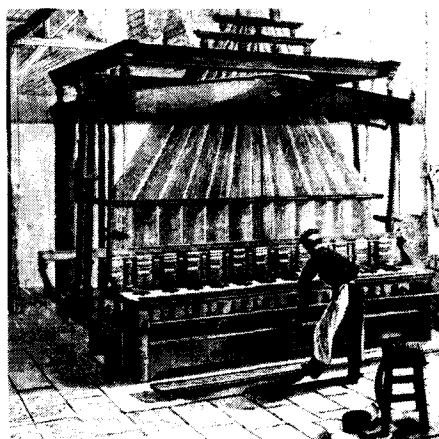
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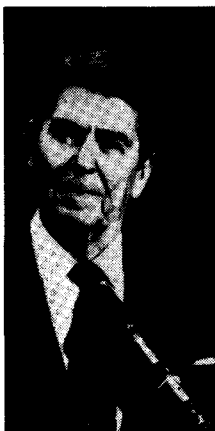
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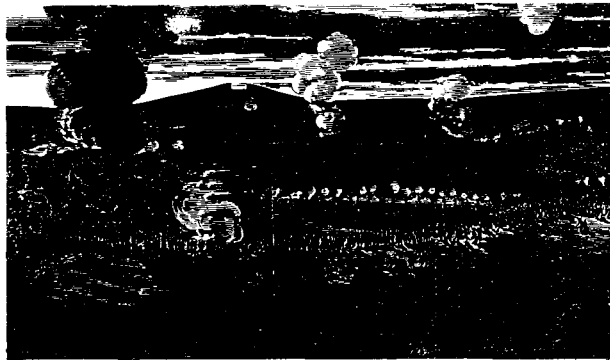
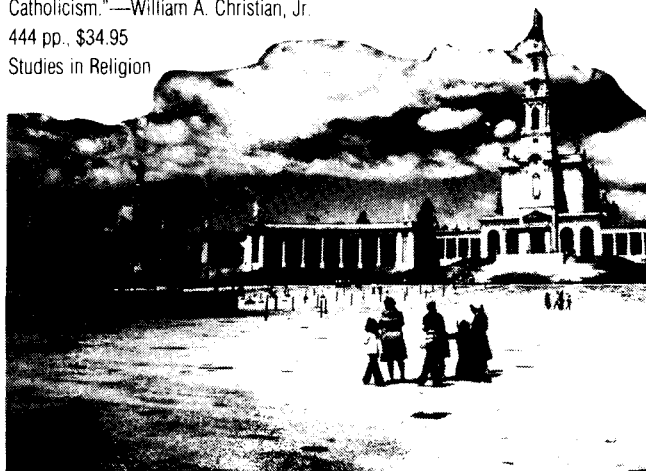
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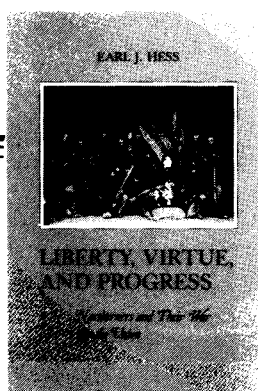
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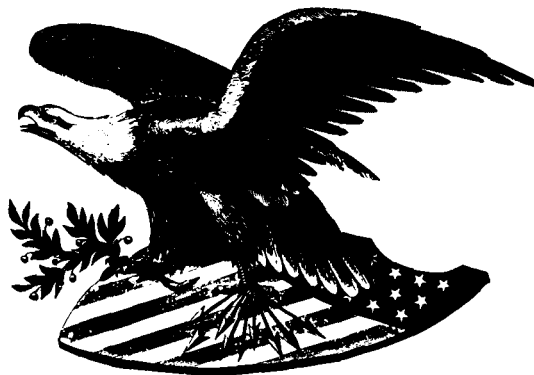
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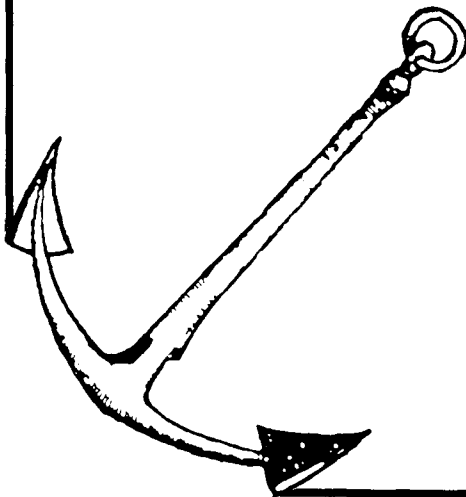
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